VOLUME XV AUGUST, 1923 No. 1

BIOMETRIKA

SIR THOMAS BROWNE: HIS SKULL, PORTRAITS, AND ANCESTRY.

BY M. L. TILDESLEY.

INTRODUCTORY NOTE.

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ON January 26, 1922, the skull of Sir Thomas Browne was brought from Norwich to the Museum of the Royal College of Surgeons by special messenger. With the skull came a letter from Sir Hamilton Ballance, written on behalf of the staff of the Norfolk and Norwich Hospital, informing me that it was to be restored to its ancient resting-place under the floor of the chancel of St Peter Mancroft, Norwich, but before this was done the staff desired me to make an exact and permanent record of its form and features.

At the time this task was entrusted to me it was the good fortune of this Museum to have attached to it Miss M. L. Tildesley, a research worker, carrying out investigations under the aegis of the Department for Scientific and Industrial Research. She had been trained in the exact methods of the Biometric Laboratory of University College and had already made an excellent contribution to the literature of racial craniology. After successful plaster casts had been made of the outward form of the skull and of its brain cavity by Mr William Finerty, foreman in the Museum, I set out to make an anatomical examination and soon realized that this was the least part of my task. Every detail in the history of the skull had to be reinvestigated to make certain of its authenticity. It became necessary to collect and collate all facts relating to portraits and representations of the great Norwich Physician. Registers, libraries and private collections had to be searched for data bearing on his personality and on his lineage: in order that this part of the undertaking might be thoroughly done I enlisted the co-operation of Miss Tildesley. She set out on her part of the work with enthusiasm, skill and intuition, following up clue after clue, placing any doubt at rest as to the authenticity of the skull. Portrait after portrait and engraving after engraving she traced to its source; she unravelled many obscure threads of Browne's lineage. My part of the partnership lagged; first a multiplicity of engagements delayed me and then finally, a long and serious illness compelled me to hand the entire task over to her able hands. And

so it has come about that what I undertook to do has devolved in its entirety on Miss Tildesley.

And now, when I come to read over the proofs of her monograph on the skull of Sir Thomas Browne, I have the pleasant conviction, one which I am sure its readers will share with me, that my illness, for them, has really been a blessing in disguise. It has given Miss Tildesley the opportunity of manifesting an ability for research of a high order; throughout this monograph she pursues the truth with a restless and logical diligence. We have here exemplified the best kind of scholarship—one which illustrates the right application of laboratory methods to historical enquiry and to anatomical pursuits. If the skull of Sir Thomas Browne has now passed beyond the possibility of further enquiry this monograph will remain to give subsequent generations of his countrymen a faithful record of what the outward appearances of the Norwich physician really were.

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(I) INTRODUCTION.

Early in 1922 the skull of the seventeenth-century physician and great man of letters, Sir Thomas Browne, was sent to the Royal College of Surgeons of England in order that records of it might be made and preserved for posterity to examine when the skull itself should have been restored to its original resting-place. Casts of the cranium and lower jaw, and casts of the brain-cavity have accordingly been made; photographs and drawings and exact measurements have been taken; and the skull, returned to Norwich, was duly re-interred within the Church of St Peter Mancroft on July 4th, 1922*. It will be asked by many what use our own day, or posterity either, can make of such records: and if they do anything further than satisfy human curiosity about famous men of the past. It will therefore not be inopportune to state here as definitely as possible what purposes can be served by records of the skull of Sir Thomas Browne.

Every skull of known race and period is useful, of course, as a contribution to the material required for the study of racial characters. The aim of this study is to determine with all possible accuracy the physical characters of the numerous races which inhabit, or have inhabited, the globe; to determine also their physical relation to one another, and, from this, somewhat of their past movements and wanderings; and to measure the degree to which various racial characters in the mental and physical domains tend to be associated together.

A very great amount of material is required to render such investigations possible. Considerable progress has already been made towards accumulating this, in the osteological collections of many countries: even so, the material already gathered can be looked upon as little more than a beginning. Seventeenth-century England is more fully represented than many other periods and many other lands, but even in this case there is room for more. The records therefore of the skull we are now considering have firstly a certain usefulness for the purpose of racial study.

But there is another study, akin to this, and provocative of far greater interest to the majority of men: it is one which seeks to answer the question: To what extent do various mental and physical characters tend to be associated together *in the individual*? Can the body really tell us anything about the mind of a man, and if so, how much?

* Copy from the Register of Burials in St Peter Mancroft Church, Norwich, Book No. 4, page 37, Number 292.

Name	Abode	When buried	Age	By whom the ceremony was performed
The Skull of Sir Thomas Browne	Since 1845 † Norfolk and Norwich Hospital Museum	1922 July 4	317 years	F. J. Meyrick Vicar

† The date when the skull entered the Museum was more probably 1847: see our p. 40.

Numerous confident answers have been given, of course, and with great detail, by the palmist, the physiognomist, and the phrenologist, the last-named basing his conclusions upon the contours of the skull. Our faith in the neat charts in which he indicates and delimits the seats of some thirty-five mental and moral qualities, which divide up the surface of the brain between them, has, however, been rudely shaken by the newer charts which in our own day very different methods of research are slowly filling in. His methods are empirical, and have proved misleading. But was his basic idea that it was possible to establish some logical connection between the inward and the outward entirely at fault? The answer to this will depend upon the degree of association found to exist between:

(1) the mental faculties and the proportions of the various parts of the brain; and then on the degree of association, doubtless getting weaker at each remove, between

(2) the mental faculties, and the modelling of the brain surface,

(3) „ „ and the contours of the cranial cavity,

(4) „ „ and the outer surface of the skull,

and lastly

(5) the mental faculties, and the living head.

To determine any one of these correlations, it is obvious that the investigator needs to be closely acquainted with the mentality of each individual studied. It is only seldom that this is known to him, except when he is dealing with the living head, and this is just where we should expect the degree of association to be slightest, and research to be least fruitful of result*. The pairs of characteristics which it is most desirable to study together are precisely those concerning which the data are fewest. To this branch of science, therefore, our records of Sir Thomas Browne make an especially valuable and welcome contribution, since here we know the cranial cavity from our casts, and the mind of the man from his books. Many more such records are needed, for however suggestive the study of small numbers may be, the conclusions based upon them are tentative at best, and it will probably be left to a later generation to answer our questions fully and certainly, by means of material which we must help to provide.

Two ways have now been indicated in which the skull-records of Sir Thomas Browne will be of value. They need not wait, however, for later years to prove their usefulness. Theory must and will leap ahead of knowledge, and needs continual testing. Such records as we have here supply a means of testing scientific theories; and not scientific theories alone, but common beliefs which pass currency for the most part unquestioned. It is almost a dogma in popular belief, that a receding chin reveals weak character, that the man whose eyes are close together is not to be

* Considerable research has in fact been undertaken in this direction, notably by Prof. Karl Pearson on 1000 Cambridge undergraduates (*Biometrika*, Vol. v. p. 105) and by Dr Chas. Goring on 5000 English criminals (*The English Convict*, pub. 1913). The result has been to demonstrate the absence of any degree of association of prognostic value between those mental and craniometrical characters with which the investigators dealt.

trusted, and that a low and receding brow involves a low mentality. Statistical investigation of the living can, and no doubt soon will, show whether any degree of association exists between these pairs of characteristics, and also give a measure of their tendency to be found together. Meanwhile it is quite useful to test by single examples such as the one before us whether our rules are quite as infallible as we are tempted to suppose.

More unconsciously however, doubtless more truly and to a far greater extent we base our mutual judgments upon the expression of the living face, upon features which are not determined by the shape of the bony framework alone.

We do not feel we know a man unless we know what he looks like, and our curiosity is very strong in the case of men dead and gone whom we have learnt to know in part through their actions or their works.

The faces of most of our great men are known to us, if known at all, at second-hand through the medium of an artist—perhaps of several artists, of various schools, of varying skill and truthfulness, their work coloured by differing individual mannerisms. Or it may be that we get the face at third-hand or fourth. The influence of any of these factors may render the artist's work less faithful in the photographic sense.

The various results, considered merely as evidence, are apt to be rather confusing. If we place them side by side, we realise how imperfectly some of the portraits at any rate must have transmitted the exact appearance of the original: if one speak truth on this point the others cannot, but which is the truest?

That is a problem that we now have an opportunity of solving to some extent in the case of Sir Thomas Browne. If we could also have the skull of Shakespeare before us for a short time, what vexed problems that too would resolve! The skull alone cannot tell us all we wish to know about the living head, but the skull and the portraits together can do much.

We propose to deal with our two kinds of evidence, in the following order. First of all we shall study the portraits independently: all portraits whatsoever, in so far as we have been able to trace them, that make any claim to represent Sir Thomas Browne will be dealt with and their credentials examined. In the course of this, there will be some sifting of copies from originals, and those which can be looked upon as evidence will be reduced to a comparatively small number. We shall next examine the bona fides of the skull itself. Having satisfied ourselves as to that, we can study its characters and then compare it with the portraits which have survived the sifting process, using it to determine which of them conforms most closely to the details which the skull reveals. It is these portraits which will, presumably, portray most truthfully those details in which it cannot guide us.

(II) THE PORTRAITS.

The would-be student of the portraits of Sir Thomas Browne is usually referred to three well-known paintings, one at Norwich, in the church vestry of St Peter Mancroft, one at the Royal College of Physicians, London, and one in the Bodleian

Picture Gallery at Oxford. It is sometimes confidently asserted that all three are original and contemporary*. We now know, however, that the last of these three was painted half a century after the death of the subject; and since definite evidence is lacking as to the date of the other two, we are perhaps on safer ground if we begin first with a study of the engraved portraits, dated by the publications in which they appeared.

(*a*) *Early Engravings.*

The earliest engraved portrait of Sir Thomas Browne to which we can find any reference, is one mentioned by Henry Bromley in his *Catalogue of Engraved British Portraits* (1793) as being in "the folio edition of *Religio Medici* 1663." This 1663 folio edition with portrait is again referred to in Lowndes' *Bibliographers' Manual* (1834) and to this information H. G. Bohn added in his reprint that the edition was surreptitious. Again, in Jan. 1881, replies were given in *Notes and Queries* to an enquiry concerning early portraits of Sir Thomas Browne: one correspondent, Wm. H. Peet, writes: "There is a good portrait in the folio edition of *Religio Medici*, published in 1663" (*Notes and Queries*, 6th series, Vol. III. p. 58); another, J. Ingle Dredge, says: "There is an engraved portrait of 'Thomas Browne, med. doctor' in a small half-sheet. This was published before he was knighted by Charles II in Sept. 1671" (*loc. cit.* p. 57). With regard to the latter statement we might suppose either (1) that the "half-sheet" referred to was taken from the folio edition of 1663, or else (2) that the portrait in this edition was first published separately. Certainly J. Ingle Dredge's description does not apply to the only engraved portrait published *before 1671* with which we are acquainted, for this was in quarto, and bore a different inscription (see p. 7); and the only *folio* portrait we know was published in 1686, with again a different inscription (see p. 8).

Here then we have statements from five different quarters to the effect that an early portrait engraved on a folio sheet existed; and the place indicated for it by three of these, is a 1663 fol. edn. of the *Religio Medici*. Now this edition is not mentioned in any careful bibliography of the *Religio Medici* yet published†; it was not known by the late Sir Wm. Osler, who believed he had a complete collection (with one exception: the Dutch edn. of 1688) of the editions of this book; Dr Geoffrey Keynes, who has in course of preparation a bibliography of all the works of Sir Thomas Browne, has never heard of it; and extensive enquiries on our part have failed to bring it to light‡. One is tempted therefore to suppose that all these references originated in a mistake made by Henry Bromley, and

* See *Notes and Queries*, July 13, 1895.

† The most recent and the completest of these is that by Mr Chas. Williams, 2nd edition, pub. at Norwich, 1907.

‡ It may save possible future searchers after this edition some labour if I here detail the places in which it was *not*, in 1922:—the libraries of the British Museum, Royal College of Surgeons of England, Royal Society of Medicine, Royal Society, Royal College of Physicians, Guildhall, Medical Society of London, Cambridge University, University College (London), the London Library, the Bodleian, the John Rylands (Manchester), and the Cheetham (Manchester). Enquiries published in *The Times Literary Supplement*, and *The Clique* (a bookseller's journal) have been barren of result.

echoed later by the other four writers. Our doubts may be strengthened by the reflection that *Religio Medici* was a short work to publish alone in folio. Against this theory, we have the fact that of the three later witnesses each provides a detail given by himself alone, and not copied from Bromley: Bohn that it was "surreptitious," Peet that it was a "good" portrait, Dredge that it is in "small" folio, and without any reference to its being in a book. We must be content, therefore, having put our evidence on record, to leave the probability of the existence of the 1663 edition an open question.

The earliest engraved portrait of Sir Thomas Browne which we *know* to have been published was prefixed to the 5th edition of the *Pseudodoxia*, quarto, in 1669. This is so unlike all the other portraits that one is unable to exclude the suspicion that it might have been one of the forgeries not uncommon in those unscrupulous days, when the trick of re-naming the previously published portrait of some other man was frequently practised on the public. In some cases an engraved plate was taken from stock—doubtless the one most nearly resembling the new subject—and merely printed with a new inscription. In others the plate was further revised by superimposing later work on portions of the original; or where this was impossible, by first removing some details in order to engrave afresh. A noteworthy example of this kind of faking is the equestrian portrait of Oliver Cromwell by P. Lombart, which finally emerged, after five transformations, as that of the Royal martyr himself*!

A close examination of the portrait in the 5th edition of Browne's *Pseudodoxia* suggests that this is another example of the same method (see our Plate VII, Fig. 9). There are tell-tale signs of the original engraving having been erased, round about the left side of the head. The outline of the cheek was probably altered, the curls filled in as they are now, and the portion of the cliff behind roughly hatched in. Some day we may identify this figure in its earlier incarnation. But however all this may be, the portrait was replaced in the next edition of the *Pseudodoxia* (again 4to.) three years later by one entirely different, and one which we can believe to be more authentic: the portrait of 1672 bearing the signature of Van Hove. The "1669" portrait was unsigned. It may be the engraving referred to as "Cross, 4to." in Edward Evans' *Catalogue of Engraved Portraits* (1830...). In the opinion both of Mr C. F. Bell and Mr H. Breun—authorities whose opinion carries weight—the portrait is in the style of Cross and was probably attributed to him by Evans on internal evidence alone. It was stated by J. Herbert Slater, a well-known authority on books and engravings, to be by Van Hove, on what grounds we do not know†. Simon Wilkin quotes Mr Ottley (Brit. Mus.) as attributing it to John Dunstall‡. It has never been reproduced in any subsequent edition.

* See *The Headless Horseman*, by George Somes Layard. (Philip Allen & Co., London, 1922.)

† This statement was made in an Answers-to-Correspondents column, in reply to a question by Dr George Peachey. I have seen the cutting which Dr Peachey has preserved, but it is difficult, after the lapse of some thirty years, for him to supply the exact reference to the date, etc. of the periodical.

‡ Sir Thomas Browne's *Works*, 1835–6 edition, Vol. II. p. 167.

We now turn to consider and compare the next three portraits published. They are

(1) The engraving which bears the name of the Dutch artist Van Hove (sometimes called Van den Hove, *circa* 1630–1715*), forming the frontispiece to the 6th edition of the *Pseudodoxia*, 4to. in 1672, ten years before the death of the author (see our Plate VII, Fig. 10).

(2) That inscribed P. Vandrebanc (1649–1697), which accompanied the posthumous publication of *Certain Miscellany Tracts*, 8vo. in 1683 (limited edition) and 1684 (see our Plate VIII, Fig. 11).

(3) The engraved portrait bearing the name of R. White (1645–1704) which adorned the folio edition of the *Collected Works* published in 1686 (see our Plate VIII, Fig. 12).

As regards the dates of the engravings themselves, Van Hove is said to have worked chiefly for booksellers†, and we may therefore assume that his portrait of Sir Thomas Browne was executed expressly for the 6th edition of the *Pseudodoxia*, which appeared in 1672.

Vandrebanc, or as he is more usually called, Van der Banck, did not come to England until about 1674‡. His engraving of Sir Thomas must in any case be given a later date than that, and I have, in fact, found nothing to encourage a supposition that either his or White's was published in any form previous to its appearance as frontispiece to the book named. The condition of the prints that have been examined indicates that, when they were struck off, the plates were in good condition and had not been re-touched. Such would not have been the case had these plates been used already for a previous issue. We may conclude, therefore, that both Van der Banck and White engraved their portraits of Sir Thomas Browne to serve as frontispieces to *Certain Miscellany Tracts* (1683) and *The Collected Works* (1686) respectively, and that these dates may be accepted as approximately the dates of the engravings too.

It is interesting to note that these three engravers represent three different schools, the Dutch, French, and English, a difference which is reflected in their work. Van Hove is not, indeed, a very accomplished representative of his school, and his training must have been fairly brief, for he was working for the booksellers by the age of about eighteen. Van der Banck, on the other hand—of Dutch extraction, but born in Paris—had the advantage of studying under the distinguished French engraver, François de Poilly, until he left France for England at the age of twenty-five. Robert White also served a long apprenticeship under a master in his art, David Loggan.

If we compare the three engraved portraits by these different men we notice a very considerable correspondence between them. The same arrangement of the hair, the same long and carefully-parted moustache, the same pointed beard, parted

* Date of death as given by Müller in his *Künstler-Lexicon*.

† Bryan's *Dictionary of Painters and Engravers*, 1910, Vol. v. p. 238.

‡ Horace Walpole's *Catalogue of Engravers*, 1794, p. 128.

down the middle—very decidedly in Van Hove's and White's, but with a parting suggested in Van der Banck's too; the same type of cloak with its broad collar seen covering the right shoulder; the same plain buttoned coat and plain linen collar, with two edges touching in front to preserve the square outline of the whole. These similarities might, perhaps, be explained by saying that Sir Thomas Browne was very conservative in his manner of dress, so that such details as have been instanced would appear in various portraits, dating from different times. But there are other details which cannot be explained in this way. In all three portraits the position is identical: all three are represented as seen rather from the right of the sitter, whose eyes are turned more in the direction of the artist; all have locks of hair straying over the forehead on the right side. We note the similar outline of the hair and the identical little curl which stands out from the rest on the extreme right, at the level of the eyes; and in Van Hove's and White's, the linen collar turned up at its right-hand corner. These are points of resemblance which can only be explained by supposing either that the two later engravings were based upon Van Hove's, or that they were derived from one common original. We will consider the former of these alternatives.

We know from the date (1672) when the first engraving, Van Hove's, appeared, that it was a contemporary portrait, as the others were almost certainly not; but did Van der Banck and White really copy from Van Hove? It is hard to believe so. One is accustomed to strange changes in a physiognomy as it passes from one brush or pencil to another, and to considerable innovations in the matter of dress. But even when we make allowance for the influence of the different schools in which the artists were trained, the number of differences here, especially between the engravings of the Dutch and French artists in spite of the details they share, point much more strongly to the existence of some source common to them all. If each went direct to this, we should expect to find that it lacked the details of dress which were supplied so variously by the three engravers.

Again, we remember that at the time the earliest of them was executed Sir Thomas Browne was in his sixty-seventh year. Now Van Hove was not a great artist, but if we can rely on the evidence of his engraving at all in the matter of age, it certainly seems to represent a much younger man. The rounded cheeks and clear-cut lips could hardly belong to a man well over middle age. Thus the second of our alternatives seems the more likely, and we are directed further back in our search for a more original record.

If we turn in our quest to the fine collection of miniatures in the possession of the Duke of Buccleuch I think we have found one!

(*b*) *The Buccleuch Miniature and the Engravings compared.*

The miniature which bears the name of Sir Thomas Browne in the Duke of Buccleuch's collection is painted in oils on silver, and measures $1\frac{5}{8}'' \times 1\frac{3}{8}''$: our reproduction (Plate I, Fig. 2) shows it somewhat enlarged. The face and shoulders are turned to the observer's left, instead of to his right as in the engraved portraits, a difference which we were prepared to find in the original of these 17th century

engravings. In the latter half of the 17th century Wm. Faithorne's method of engraving from the original seen through a mirror was not used by all. By this method the engraver obtained on his plate a copy of the picture in reverse. This, of course, became again reversed in printing, with the result that the print and the original would face the same way. Until the adoption of this device became general, it was customary to produce the engravings in reverse, the more difficult alternative being to reverse in process of drawing on the plate.

In the miniature we find at once all the features which we had noted as common to the three engraved portraits: the outline and arrangement of the hair, the wisps hanging over the forehead, the same little curl already referred to, the same square white collar with the turned-up corner, the moustache framing the mouth, the imperial parted down the middle. The small dark spot seen just above the inner corner of the left eye is due to damage or discoloration. Near the edge at the bottom of the miniature, the oil-paint appears on a close examination to have peeled off and been restored, the largest patch reaching nearly to the turned-up corner of the collar. The part of the cloak which has not been disfigured does not show any detail.

In the three engraved portraits the cloak is carried further down to the level of the waist, but one point common to all three—the outline of the cloak at the shoulders—will be seen again in the miniature.

The history of the Buccleuch miniature is one I have been unable to trace, and since its authenticity as a contemporary portrait* and the probable parent of most of the long series of representations of Sir Thomas Browne which have been produced over two and a half centuries depends chiefly on internal evidence, it is worth while to compare it in more detail with the engravings whose dates are better known.

First with the Van Hove (Plate VII, Fig. 10). The similarities which connect this engraving with the miniature as a different version of the same thing have already been pointed out. But an engraving, apart from the details which an engraver frequently sees fit to add or modify, does not usually reproduce a picture with photographic faithfulness. There are differences; and it is profitable to examine these. The Van Hove engraving is harsher, harder, and more angular than the miniature, with a harshness not all involved in the different medium used, as the Van der Banck will show. The curve of the upper lip is exaggerated into a sharp angle; the eyebrows are heavier; the soft wisps straying over the forehead have become sharply outlined tails of hair; the pointed beard, brushed to right and left in the miniature, is reduced to an imperial drilled into a severe and rigid parting that nature unassisted could not possibly maintain! The eyes are larger and more sombre; the whole expression is forbidding, whereas that of the miniature is sweeter, and accords better with what we know of the character of the philosopher; more of the cloak is shown, and its folds are stiff and rigid.

* The Director of the National Portrait Gallery gives its date, from internal evidence, as 17th century or possibly early 18th century. He pronounces the artist a painter of ability, though not of outstanding distinction.

We now turn to the Van der Banck (Plate VIII, Fig. 11), engraved for a smaller book, 8vo. size. Here we have an engraving characterised by a softness of touch which, Horace Walpole tells us*, won for the work of this artist the popularity it enjoyed in his own day. The composition of the face is, however, less admirable: it is a weak face, and in this it differs from the miniature. When we compare it in detail with the latter, we note that the backward slope of the forehead is considerably modified, the face is longer, the eyebrows less firmly marked, the eyes lighter in shading, the parting in the beard retained only in its upper half. Van der Banck has his own interpretation of the indefinite hair on the forehead, differing from Van Hove's. Unlike the latter he retains the hair which we see on the cheeks of the miniature, and follows more faithfully the line of the nose. He introduces a variation of his own in the collar, narrowing it and straightening out the turned-up corner, and he fills in the details of cloak, which the miniature lacks, without reference to the earlier engraver.

We see, in fact, that whatever these two engravings have in common they have also in common with the miniature; where they differ from it, they differ in opposite senses.

We next examine White's engraving (Plate VIII, Fig. 12). This, prepared for the first folio edition of Sir Thomas Browne's *Collected Works*, is of course larger than either of the others. It is seen at once to be the work of a more accomplished artist. The parted beard is again shown, parted more like Van Hove's though not quite so severely. On the other hand, there is hair on the cheeks, as in Van der Banck and the miniature; the face is somewhat longer than in the latter, but not so long as in Van der Banck; the eyes, unlike those of both the other engravings which differ in opposite ways, are of the proportions and depth that we see in the miniature, and have more of the latter's light in them; the interpretation of the strands on the forehead is that of a gentler Van Hove; in the folds of the cloak White differs from both the other engravers, except that the general arrangement suggests Van Hove's, which he no doubt had before him. Again, he has been kinder to the slope of the receding forehead, than are Van Hove and the miniature-painter.

Altogether it is impossible to conclude that White copied exclusively either from Van Hove or Van der Banck, and if he had merely attempted to reconcile these two, he would probably not have achieved anything so much like the small portrait in oils. It is worth while to note Horace Walpole's comment on White's work as a whole, that "what distinguished him was his admirable success in likenesses†." This would hardly be true in the present instance if either Van Hove or Van der Banck had supplied him with his original.

We can reject at once any attempt to explain similarities between the painting and the engravings, by the theory that the painting was later, and based on one of these, for it is most unlikely that the painter would ever reverse the picture he was

* Horace Walpole's *Catalogue of Engravers*, 1794, p. 129.
† *Ibid.* p. 136.

copying. (Incidentally, it may be mentioned that the same fact rules out these engravings as possible originals of any of the *painted* portraits which will be considered later.)

The hypothesis which best survives our examination is that the features common to the engravings are accounted for by the circumstance that each engraver found them in the original from which he copied. We might, therefore, naturally conclude that each copied direct from the miniature. But we must not overlook another possibility: the engravers may have copied from non-identical portraits if these were closely related and showed the same characteristics as are reflected in the engravings. Whether any other portraits as yet discovered can dispute with the miniature the title to which it has made out so reasonable a claim, will be seen in the course of our examination of the others. There is however one more contribution to the subject which the work of the engravers might yield, before we pass to other portraits. We have been content to accept for all three engravings an original facing the reverse way, on the ground that such reversal is frequently found. But it occurred to me that one would be on safer ground if one enquired into the actual practice of the individual engraver. To attempt to assemble data concerning all or even a large proportion of the work of the artists in question would have been an impossibly lengthy undertaking under the circumstances. I did however succeed in bringing together engraving and original in the case of five portraits engraved by Van Hove, one by Van der Banck, five by White, and four by Loggan, White's master. Van Hove's all reversed their originals, Van der Banck's also reversed; White's and Loggan's all reproduced the originals unreversed.

Now this evidence, while certainly not strong enough to support the assertion that these artists *always* did as they have done in these few instances, does at least predispose us to expect that the original of Van Hove's engravings would be a portrait facing to the left, as the miniature does. As regards White, we should expect, from this, that his original would be turned to our right. We do not, however, on the evidence forthcoming as yet, find any difficulty in making the miniature his probable original—for the discovery of a single instance in which White reversed would negative the inference drawn from the five in which he has not—but in his case we sit rather more loosely to our conclusions*.

(*c*) *The Portrait at the Royal College of Physicians* (Plate II, Fig. 3).

This portrait (24″ × 20″), bearing the inscription "Thomas Brown Eques Auratus" in large letters at the top, is painted upon an oak panel. It has no history that has been traced as yet. The name of the painter is unknown; so also is the date of the painting. The only clue to the date at which it came into the

* [It confirms the evidence which, I think, exists in the engravings themselves, that White had not the miniature before him, but simply copied Van der Banck; the wisp of hair curling to the right at its tip is not in Van Hove or the miniature, but it is in Van der Banck. In Van Hove the buttons are on the left side of the parting line of the vest, in Van der Banck and White they are on the right. He may have had Van Hove also, but I think there cannot be a doubt that he certainly had Van der Banck. Ed.]

possession of the Royal College of Physicians is to be found in the fact that Dr Edward Browne, Sir Thomas' eldest son, was President of the College from the year 1704 till his death in 1708, and it is presumed* that the portrait was his gift. If we examine the portrait we recognise at once some familiar traits. The same position as in the miniature and not reversed as in the three engravings; the streaks of hair on the forehead, the square collar with the corner turned up, the pointed beard. The beard is not parted as before, and though the general outline of the hair is much the same, the little curl which breaks through on the right in the other portraits (on L. in engravings) is scarcely indicated. This portrait is carried down to the level of the waist. It was very difficult however to distinguish any details of the dress except the central line of buttons until the picture was taken down from the walls, and could be inspected without the interposition of the glass which covered it in the frame. Fortunately it is not long since it was cleaned, so that such details as exist are not obscured by a veil of dirt.

I have examined the dress carefully, and can make out the folds of the cloak as it is gathered together by the two arms. The hands, covered with brown gauntlet gloves, are clasped in front. These details of dress and figure are merely sketched in, in dark colours, and it may be that the difficulty of distinguishing them caused the two engravers of this picture†, while preserving the buttons of the coat, to improvise a very different cloak.

The points of resemblance between this portrait and those we have just examined are however so marked that there can be no doubt we have here another member of a family of portraits of which one was the parent, and the others the offspring. In what relation does this stand to the others? Is it possible that this was the original portrait—that the miniature and the engravings by Van Hove, Van der Banck, and White were all copied from it? I think not. I ask the reader to look at this towering forehead, this conventional and rather wooden countenance, long and narrow: high foreheads were no doubt as popular then as today, and what artist, copying from this, would give his own production the low and sloping brow found in the miniature and Van Hove, or even reduce the height of it to the dimensions found in Van der Banck and White? Who would be likely to read into this face the life and expression found in the miniature, or turn its lips with their clear-cut outline of a boy of twenty, into the older mouth of the miniature portrait? Again, it is easy to understand how such different mouths as Van Hove and Van der Banck show could have had their origin in the miniature, but not easy to believe that anyone would translate that of the Royal College of Physicians' painting into the one represented by Van Hove. The more one studies these portraits, the more obvious it becomes that the painting at the Royal College of Physicians is the expression of the artist's own idea of what it would have been more becoming in Sir Thomas Browne to have looked like. Whether he was right or no, it is certain that he has nearly improved him out of all resemblance to

* *Roll of the Coll. of Phys.* Vol. I. p. 305.

† See list of portraits on our pp. 73–4.

himself if the miniature be near the truth. And on this point it will no doubt be safe to fall back upon the same canon as obtains in literary criticism: where two versions differ, the more awkward and difficult is the correct one. I am afraid we must accept Sir Thomas Browne's disturbing forehead.....!

(*d*) *The Bodleian Portrait* (Plate II, Fig. 4).

This portrait (29″ × 24″) is one which possesses a definite historical record. In editing Anthony à Wood's *History of the Colleges and Halls in the University of Oxford* John Gutch (p. 968) gives a list of eight portraits of doctors in the Bodleian Portrait Gallery, painted by J. Wollaston in 1734*, and presented by Humphrey Bartholomew of University College, M.A., in 1735, which "hung some time in the anatomy school." Mrs Poole, who has done so much valuable research work in connection with the Oxford Portraits, informs me that of these eight portraits, one, Thomas Wharton's, has disappeared; also that there is a second record later of the gift of a portrait of Thomas Willis—one of the eight—whereas only one portrait of this doctor is now in the Bodleian Gallery, so that it is not certain which portrait of him was preserved. The remaining six are each represented by a picture in the Bodleian collection†, and among them is Sir Thomas Browne. Mrs Poole adds that it is evident the portraits are copies of the stock likeness of each doctor.

If we look for the original of the portrait in which we are now interested we have not far to seek, for the shading on the white collar of the Royal College of Physicians' portrait corresponds exactly with this before us. The two paintings are indeed very much alike. A few changes have been made: thus the stray locks over the forehead have become rather more attenuated in the Oxford portrait, the mouth has acquired more of a rosebud effect, and apparently Van Hove or White has been referred to as well, and is responsible for the restoration of the very definite parting in the imperial, and in its full proportions, the silhouetted curl near the right eye.

Mr C. F. Bell, Keeper of the Department of Fine Art at the Ashmolean Museum at Oxford, has been so good as to examine the dress as shown in this picture. He reports that "the clothes are a plain surface of black....No folds are visible, and the picture was not long since cleaned and is not darkened or obscured." In the picture on the walls of the Royal College of Physicians, as we have seen, details of the cloak are present but are not clear even now, while a thirty years' film of dirt would make them quite indistinguishable.

* The picture was loaned to the Manchester (Art Treasures) Exhibition of 1857, and the name of the painter given in the catalogue slip is "Robert Walker" (a painter who died about 1660). I have it on the authority of the Director of the National Portrait Gallery that it is not always wise to attach too much importance to such a statement. The organiser of an Exhibition is very often obliged to accept and print the name of the painter to whom the lender of the picture attributes it, unless he can prove to the lender's satisfaction that it is by someone else. The catalogue entry has, however, misled Mr Algernon Graves into including the Bodleian Sir Thomas Browne in a list of Walker's works: *Century of Loan Exhibitions*, 1813—1912, Lond. 1914, Vol. IV. p. 1597.

† See the *Catalogue of Oxford Portraits*, compiled by Mrs Rachel Poole, 1905.

In short, a comparison of the Bodleian portrait with this, and of the Bodleian with all other known portraits of Sir Thomas Browne, leads us to the fairly safe conclusion that it was the Physicians' picture which served as Wollaston's chief model when he executed this part of Humphrey Bartholomew's commission. And where else would Humphrey Bartholomew have been more likely to go, to choose portraits of famous doctors for the anatomy school of his own university, than to the home of that profession, the Royal College of Physicians in London? Thus, if we mistake not, we get in the Bodleian portrait another descendant of the miniature—one in the second generation.

(*e*) *The Norwich Portrait* (Plate III, Fig. 5).

The painted portrait in the vestry of St Peter Mancroft, Norwich, was presented to that parish* in 1739† by Dr Edward Howman, whose father purchased the house of Sir Thomas Browne in Norwich after Lady Browne's death in 1685‡. History takes us thus far back. It is not known how the painting came into the hands of Dr Howman's father, but it is known that the latter was not a newcomer into Norwich§, that he had been a colleague of Sir Thomas Browne's in that city, and presumably also a friend.

In examining the Norwich portrait, we realise at once that this face seems much more alive than those of the two paintings we have just studied: it would be easier to believe of the former that it had been painted from the living head. We will however compare it, as we did them, with the miniature. There are certain obvious differences. In the Norwich portrait the hair is smoothed back on the forehead, and a different type of collar is worn. It is surely a curious coincidence, however, if these portraits were painted quite independently of one another, that they should have been painted from exactly the same angle, facing the same way, with the eyes turned in nearly the same direction, and with the light falling similarly on both heads. We may take advantage of this similarity of position to compare the two faces more in detail. The chief difference lies in the smaller relative width in the region of the eyes of the larger portrait owing to the fact that the face tapers less below the cheek-bones, and that the forehead falls back rather less abruptly. The forehead, thus heightened, stops short however of giving the monumental effect of the Bodleian brow. We note also that the Norwich nose is somewhat aquiline. These differences apart, we find a close resemblance between the two faces. The lower part of the nose, the mouth, beard, etc. are strongly alike, though on the lips of the Norwich portrait there is rather more hint of a smile. It is to be regretted that the outline of the hair in the latter has become so vague under the varnish with which the picture is now heavily overlaid, that one can no

* F. Blomefield's *History of the County of Norfolk*, Vol. IV. p. 193. (Blomefield wrote in 1741.)

† I quote here the final conclusion as to date to which the late Mr Chas. Williams came on this matter (see *Souvenir of Sir Thomas Browne*, pub. Norwich, 1905) though he does not give his authority for the statement, nor have I so far been able to identify it.

‡ *The East Anglian*, Vol. I. 1885, p. 194: from transcript of Dr Ed. Howman's marriage-settlement, 1704.

§ *Notes and Queries*, July 13, 1895, "Portraits of Sir Thomas Browne."

longer say with certainty where the hair and background meet*. At our request, however, an independent observer, Mrs Sydney Long, has drawn upon a matt print of our photograph of the Norwich portrait the outline of the hair as nearly as she can judge it from the painting. Mrs Long was not told beforehand why we wanted it, nor with what we proposed to compare it. The general sweep of the outline she has given us is the same as is seen in the miniature.

What then can we conclude as to the relation between these two paintings? Are they independent, or is one copied from the other? The points they have in common fall into two categories:

(1) Similarity of face and feature, which might be ascribed simply to the fact that the living original was the same in both cases (a consideration which could apply quite well to the hair on the face; this, unless the wearer deliberately altered its cut or style, would vary its lines very little from day to day).

(2) Similarity of position. And it would not be possible to say dogmatically, on this ground alone, that one portrait was copied from the other. The position is not unusual; nor is the lighting.

As to other evidence, all we can say concerning the outline of the hair is that it is not definitely unlike in the two portraits, and that its general sweep seems in fact to be the same.

Dogmatic conclusions either way do not seem justified by the facts as yet before us and we must leave open the question of close relationship between these two pictures. There is one other question of interest in this connection, however, to which we may be able to find an answer: Supposing one portrait to have been copied from the other, which was the original? Here the style of dress can help. The collar of the Norwich portrait belongs to the sixth decade of the seventeenth century; that of the miniature, to the decade following. We must suppose, therefore, that if one is a copy, that one is the miniature. The artist who painted the copy might bring the clothes up to date, but he would hardly be likely to change them for the style of ten years earlier.

It is hardly necessary to point out that the possibility we have mooted in no way disturbs our first conclusions as to the parentage of the engravings: it merely confers upon the Norwich portrait the title of grandfather to these lesser fry.

A few words may be added as to the later history of this portrait. It had become somewhat damaged in the course of time, and at some date during the latter half of last century was cleaned and repaired at the cost of Mr Chas. Williams. The rents which had unfortunately been made in the canvas can be traced in our photograph: one is seen above the parting in the hair, and a little to the left; two other slits, one rather higher than the other, can be seen in the hair, to the right of the eyes.

* The outline shown in the reproduction published in 1905 (*Souvenir of Sir Thomas Browne*, by Chas. Williams) is not to be found in the painting itself and has evidently been introduced by the photographer. That in Miss Frere's copy in oils (see our p. 17) is again different.

On Nov. 25th, 1871, it was transferred to the care of the Norfolk and Norwich Hospital, on the following conditions: "The Churchwardens of the parish of St Peter Mancroft, Norwich, having deposited in the Board-room of the Norfolk and Norwich Hospital the oil-painting of Sir Thomas Browne, the Board of Management hereby undertakes to return the same whenever it may be demanded by the Churchwardens of the said parish*." In the year 1905, the year when the tercentenary of the birth of Sir Thomas Browne was celebrated, the Churchwardens of St Peter Mancroft demanded the return of the picture, and it rejoined other of their treasured possessions in the vestry of that Church.

Before returning it, the Board of Management commissioned Miss Amy Frere, of Norwich, to paint a copy; and this copy now hangs in the Board-room of the Hospital in place of the original portrait.

(*f*) *The Gunton Portrait* (Plate IV, Fig. 6).

This portrait ($12'' \times 9''$) was discovered some ten years ago by Lady Suffield in a room in the servants' quarters at Gunton Park, Norfolk. Nothing is known of the date at which the picture came into the family, but the reason for its relegation to the servants' quarters is clear. It is feeble.

The figure in this picture is seated, the right arm resting upon a table, and near it two volumes. One lies flat; the second stands upon the first with its back to the observer, exposing the title *Religio Medici*. The picture is painted on an oak panel; the back is hand-sawn and rough, with bevelled edges. From the appearance and condition of the wood, and of the painting, Mr Milner, Director of the National Portrait Gallery, assigns this picture approximately to the end of the seventeenth century: it may have been a nearly contemporary adaptation, probably by a local artist.

We say adaptation, because, having features which link it closely with the miniature and the Norwich portrait, it is also poorer than either. Particularly does the artist's lack of skill become manifest when he departs from his copy, and embarks on variations that are all his own. From the shoulders downwards, the figure has been adapted to the introduction of the table, with a book upon it which indicates the identity of the person portrayed. The cloak is caught up in clumsy folds over the left arm. The right arm and hand are exposed; and if they are by the same artist as painted the face†, they afford good evidence that the original contained nothing like this, but left the Gunton artist to his own unaided draughtsmanship.

We turn then to examine more closely the upper part of the figure, with a view to identifying the portrait from which this part was copied. The pose is the familiar one; the hair, though fairer than in any of the other pictures, presents the well-known outline; and we recognise at once the little curl which breaks through just above the bush of hair resting upon the left shoulder. The moustache also, and the cut of the beard, are old acquaintances of the reader who has followed

* Minute-book of the Board of Management of the Norfolk and Norwich Hospital.

† There are indications that the right arm from shoulder to cuff may have been re-painted later.

us thus far. Many of the features we have mentioned are present in the Royal College of Physicians' and Bodleian portraits, as well as in the miniature which brought them forth; but a comparison of the Gunton portrait with these three together leads us to reject at once the idea that either of the two first-named served as its original. The Gunton face has not been lengthened out like these; and where these two introduce slight variations in the hair—such as bringing it down to the right shoulder in a solid mass—the Gunton does not follow suit. We may safely concentrate, therefore, upon the miniature and the Norwich portrait as its possible originals.

In one point the Gunton portrait resembles more closely the miniature: the nose is straight, and does not show the slight aquilinity of the Norwich nose. Again, the disposition and outline of the hair, as we have already noted, is the same as in the Buccleuch miniature: whether it is the same in the Norwich portrait we cannot now see. Certainly the Gunton hair as it frames the face is more like the Norwich. The separate strands on the forehead are smoothed back, the middle parting is shown, and no ear-tip is left visible. Again, the Gunton collar is of the Norwich pattern though its outline, on our left, has been adapted to the droop of the figure's feeble right shoulder. The tassels of the collar-tie are there too, although less distinctly than in the Norwich picture. And once more we have to ask ourselves: Is it likely that a painter copying the miniature would deliberately change the collar for one earlier by ten years or so? It is not. Another fact which is not without significance for us, is that Gunton Park is but a few miles distant from the city of Norwich. Weighing together all these considerations, we are brought to the very reasonable persuasion that we have in the Gunton portrait a local copy of the painting which is now in St Peter Mancroft, Norwich, and which, as far as we can tell, has never left the city of Sir Thomas Browne's adoption.

But to this conclusion there is a corollary. If the Gunton was copied from the Norwich portrait, then we know what the Norwich *hair* was probably like. And how does the outline of the Gunton hair come to correspond so closely—even to the silhouette curl—with that of the miniature? This surely is a detail that cannot be accounted for by mere coincidence, or by the simple fact that the pictures portray the same person? A reasonable answer to this question is not far to seek: the Gunton portrait and the miniature *both*, were probably copied from the Norwich. Here, then, we have the further piece of evidence which was needed to make us climb down from the fence on which we were left sitting a little while back; and the side on which we descend is that which would make the miniature not an independent portrait of Sir Thomas Browne, but a 1660–70 copy of the Norwich painting with the clothes brought up to date*.

* [Neither the Norwich nor Gunton portrait has the forehead curl of the miniature and of the later portraits and engravings. This with the difference of collars, and the barer upper lip below the nostrils at least suggests that the miniature may be—at any rate to some extent—an independent portrait of later date than the Norwich painting. Ed.]

(*g*) *The Plumbago Drawing, in the National Portrait Gallery* (Plate I, Fig. 1).

This portrait, recently acquired for the nation, was recognised by the Director of the National Portrait Gallery to be one of Sir Thomas Browne, through its likeness to White's engraving. It was one of three pencil portraits in the possession of the late Mr Charles Ready, who died in London in 1922. Mr Ready came of a Norwich family, and spent his early years in that city. He was an ardent collector of all kinds of *objets d'art*; it is not known how nor when the portrait came into his hands, nor whether he was aware of the identity of the subject.

This drawing was evidently composed as an oval on a rectangular piece of vellum which has been cut down. The slip of vellum now measures $4\frac{1}{6}'' \times 3\frac{7}{12}''$, and is pasted on a thin card its own size. It has no signature. Mr Milner assigns it to David Loggan (1635–*c.* 1700), who came to England about 1653.

David Loggan was the master from whom Robert White learnt the art of portraiture, both in pencil on vellum, and with the graver on copper. A collection of nine pencil-portraits by Loggan, and one of thirteen by R. White, in the Dept. of Prints and Drawings of the British Museum, will enable the student who is unfamiliar with the work of these artists to compare their styles. Loggan's work is very finished: it is distinguished by great precision and fineness of detail. This may be seen to especial advantage in his rendering of lace, ribbon, and embroidery*. White, on the other hand, when he finished the whole drawing (more often, he finished the face only, and sketched in the rest) did not give us such minute detail, nor was his drawing of the folds of a garment quite so sharp and clear. Unfortunately, the pencil portrait of Sir Thomas Browne has been rubbed considerably and has thus lost much of its clearness, so that we cannot test it on this point. Nor is the dress such as gives scope for much minute rendering of detail; but where an opportunity is given by the buttons we find there the detailed and conscientious drawing which is more characteristic of Loggan. In his drawing of hair, we see again Loggan's typical fineness and finish. It is a finish that might be termed rather mechanical: a curl is shown by a finely-drawn spiral, a wave by a series of curved parallel lines. This kind of drawing is used for the hair of Sir Thomas Browne in the plumbago portrait. White tended to freer line†, such as we see in the portrait of a gentleman which is inscribed *R. White delin. 1699*, though an earlier portrait, that of a divine, signed *R. White fecit 1680*, shews rather the mechanical hair-portraiture taught by Loggan. This latter is drawn with a coarser line than Loggan used; in the 1699 portrait however the line was fine. It is arguable that White might combine in some portrait the "mechanicalness" of the 1680 hair with the fineness of the 1699, and so produce hair such as we see in Sir Thomas Browne's portrait. It is certain nevertheless that the delineation of the hair in the latter is much more characteristic of Loggan, and points more strongly to him as the artist who produced the portrait.

* See, for example, the signed portrait of John Wilmot, Earl of Rochester (dated 1671), and that of a gentleman unknown (dated 1674).

† "There is much greater freedom," Mr Milner observed, "in White's drawing and it has a marked superiority of characterisation, essentially English, fully compensating for Loggan's superlative finish and precision."

Another noteworthy feature of this portrait is the way in which the edges of the coat and of the cloak are picked out with a clear line of light. This appears to have been a favourite device of Loggan's: we see it again in his portrait of Peter Mews, Bishop of Winchester, and in that of Mrs Baily, to choose two examples out of several. This feature, which might be regarded almost as a Loggan hall-mark, does not appear in the pencil work of White which I have examined. An indication of it may perhaps be traced in White's engraving of Browne (Plate VIII, Fig. 12).

The style of the pencil portrait of Sir Thomas Browne is thus seen to indicate David Loggan as its author; and it is definitely assigned to him by Mr Milner.

We must now attempt to find the place occupied by the plumbago drawing among the other portraits of Sir Thomas Browne. Its intimate connection with White's engraving is obvious: clearly one was copied from the other direct. And since Loggan was the master under whom White studied, and since also the plumbago drawing is a much finer piece of work than the engraving, we naturally suppose that it was the engraving that was the copy. Here however we are faced with the considerable difficulty of discovering the relationship of Loggan's drawing to the other portraits; for that they *are* somehow related is obvious. The family traits—square collar with turned-up corner, stray hair on the forehead, outline of hair, etc.—speak too clearly for the connection to be denied. But all the painted portraits that can substantiate their claim to be Sir Thomas face our left; only the engraved portraits, whose reversal can be accounted for by the process by which they are reproduced, face our right. Yet the plumbago drawing faces R., too. If it was copied from one of the paintings, say the miniature, and not from an engraving, how does it come to face the reverse way?

Or if we would make it the original *ad vivum* portrait*, and one of the others the first copy, the difficulty is exactly the same. It is in fact somewhat greater, for we then have to account for the likeness between the square-collared portraits, of which this is one, and the Norwich and Gunton portraits which show the costume of a decade earlier. There is moreover a likeness between the arrangement of the coat and cloak in Van Hove's engraving and that in the pencil drawing, which we can hardly dismiss as a coincidence. Yet we find it more difficult to believe that Van Hove, having this drawing before him, changed it deliberately (for it is not merely a poor attempt to reproduce) to his stiff and ugly garment, than it is to suppose that a better artist, finding insufficient indications of the cloak in the

* There is no difficulty as regards date in supposing the pencil portrait to have been drawn from life, by Loggan. The costume dates it as belonging to the sixties. Loggan was working at Oxford from 1665—1675 (references by Anthony à Wood, etc.); Sir Thomas Browne's eldest son, Edward, having graduated previously at Cambridge, worked for his doctor's degree at Merton College, Oxford, from June 1666 to July 1667. Possibly Sir Thomas visited his son there; possibly Loggan executed a portrait at that time. This, however, is sheer surmise. Merton College has no record of a visit from Sir Thomas, nor has Pembroke (his own college) at that time. Anthony à Wood does not refer to his visiting Oxford; yet he makes many references to Loggan, whom he knew well, and some to Sir Thomas, with whom he had some correspondence. And we know the minuteness with which he records the most trifling events of his daily life, including the people with whom he dined or had tea, and the gossip he heard.

original portrait, adopted and beautified Van Hove's scheme. Again, if White copied the plumbago drawing, how is it that he makes the head erect, as in the other portraits, but not as in the drawing? and how does he get the little silhouetted curl, which the drawing omits, in the same place as they? All these are difficult questions which have to be answered if we make the plumbago drawing the original of White's engraving.

Shall we avoid them all by supposing that it was the drawing that was the copy, and the engraving its original? If we do, we must either suppose that Loggan copied his pupil's work, which sounds improbable, or assign the drawing to White, against conclusions drawn from the internal evidence. Furthermore we must allow that the better piece of work was copied from the poorer; and notwithstanding the admitted fact that White's ability was greater with the pencil than with the graver, I find a consensus of expert opinion against this theory, and here record the fact.

It remains to suggest a third possibility: that White's was a copy from the plumbago drawing, but that he incorporated some features taken from, say, Van Hove. Also that Loggan before him—though after the death of Sir Thomas?—wanting material for a portrait made some use of Van Hove's early engraving, but, in drawing the face, went to Norwich and made a direct study from that portrait. Thus Van Hove would contribute the collar, the general arrangement of the cloak, the position facing R., the definite parting of the imperial, and Norwich would do the rest.

Some such compromise as is outlined above might be devised to evade the difficulties of the other two alternatives, though we do not suggest it is more plausible than they. But confessing ourselves unable to find any ideal and unique solution of the problem, we place our data before the reader and leave it to who will—and can—to decide*.

(*h*) *The Wellcome Portrait* (Plate IX, Fig. 13).

There is in the Wellcome Historical Medical Museum, a portrait which was bought in Norwich, about 20 years ago, as that of Sir Thomas Browne. On the back are pasted two rectangular slips of paper. The wording on one, in manuscript, is now largely undecipherable, but some years ago it was copied for the late Sir William Osler and then read, "Sir Thos. Browne, Kt. M.D., Author of Religio Medici and other learned works. Practised in Norwich." Below it is a fragment of a printed description, placed centrally:

st from which a mezzotint was aped by Simon,
ssion of Wm. Douglas, Esq. of Teddington, near
h it to its present possessor, Sir John Lister Raze,
efield, Yorkshire, for the sum of £400.

* [It seems impossible to doubt that either White's engraving was copied from the plumbago drawing or the latter from White's engraving. If the former, why was the rather graceful bend of the head dropped, and why was the familiar silhouette curl of the Van Hove introduced? If the latter, it was easy to leave out the curl and give a graceful droop to the head. White's engraving is the best of all the engraved portraits, and it is not unreasonable to suppose that some relative or admirer of Browne's commissioned White (or Loggan?) for a drawing. ED.]

It will be noticed that there is no mention here of the identity of the figure in this portrait. There are given, however, a number of other possible clues to the history of the picture, which no proper investigation could afford to neglect. These led to the surprising discovery that the picture corresponded with a mezzotint by Simon, not of Sir Thomas Browne, but asserted to be of Shakespeare!

The portrait from which it claims to have been copied was not contemporary, but was painted by Gerard Zoust, or Soest (*c.* 1605—1681), who came to England in 1651 or before*. Abraham Wivell, in his *Inquiry into...Shakespeare's Portraits* (1827), quotes various statements made by others concerning this picture, and adds notes of his own. Subsequent research appears to have contributed almost nothing to our information; so we shall find most of the little that is known conveniently assembled in Wivell's pages.

Edmund Malone, in his 1790 edition of *Shakespeare's Works* (Vol. I. p. 127), stated: "About the year 1725, a mezzotinto of Shakespeare was scraped by Simon, said to be done from an original picture painted by Zoust, or Soest, then in the possession of T. Wright, painter, in Covent Garden.....I have lately seen a picture in the possession of — Douglas, Esq. at Teddington, near Twickenham, which is, I believe, the very picture from which Simon's mezzotinto was made. It is on canvas (about 24 inches by 20) and somewhat smaller than the life."

Commenting on this statement in his *Inquiry into the...Portraits of Shakespeare* (1824) James Boaden says: "Not very long since, the proprietor felt inclined to sell this picture, if he could obtain one hundred guineas for it; and Mr Sotheby, I remember, put it into one of his catalogues. He differed with Mr Malone as to its size, calling it a canvass, 20 inches by 16. He adds, 'This fine and extremely interesting portrait has been in the possession of the family of the present proprietor for upwards of a century'." Mr Boaden goes on to differ from Malone's identification of the picture with Simon's original.

Again, Abraham Wivell, commenting in 1827 on the above remarks by James Boaden, says: "Mr Douglas informs me, it never was his intention to part with this picture for a less sum than five hundred pounds. He employed Mr Sotheby to sell it, if possible, for the above sum, accordingly it was put up at the end of a sale, and bought in at one hundred pounds. Mr Douglas has presented me with the following memorandum, as to where it now is:—'The present possessor, Sir John Lister Raye†, Bart. of the Grange, near Wakefield, Yorkshire, purchased it of me for four hundred pounds. (Signed) William Douglas'." He adds that Mr Douglas tells him the picture had been in his family about 60 years (Sotheby's "upwards of a century" was evidently rather a generous way of expressing this!) and that its previous history was not known; also that Garrick and Sir Joshua

* The earliest dated English portrait is 1651, and one undated presumably belongs to 1646. Zoust probably came to England about 1644. See *Lely and the Stuart Portrait Painters* by C. H. Collins Baker, 1912, Vol. I. pp. 196—208.

† Evidently Wivell's mis-reading of Kaye as written in Douglas' handwriting. The Lister Kayes are a well-known family. An application for information as to the present whereabouts of the picture met with no reply.

Reynolds had much admired it; and that he personally agrees with Boaden in thinking it was *not* the original of Simon's engraving. He also suggests that Simon's inscription may mean that Zoust painted a copy from the "Capital Picture in the collection of T. Wright," and that he (Simon) engraved Zoust's copy. He then says: "There is in the possession of Mr Booth, Bookseller, a small copy, in oil, by Mr Cosway from the above picture, or print, which was purchased at his sale for nearly the sum of twenty pounds. The artist has taken some liberties, one in particular, by making the outline of the nose quite straight."

Thus we learn of the existence in 1827 of the following three portraits: a mezzotint by Simon, claiming to be taken from a portrait of Shakespeare by Zoust; an oil painting on canvas (20″ × 16″) (by some identified as the original of the mezzotint, by some not) in the possession of Sir John Lister Kaye; and a small straight-nosed copy in oils by Richard Cosway "from the above picture or print" in the possession of Mr Booth, Bookseller.

To-day the following portraits are known to exist: the mezzotint by Simon (of which a good pull is in the Prints Dept. Brit. Mus.); a picture in oils on canvas (20½″ × 16½″) said to be a copy of the Zoust, and attributed to Sir Joshua Reynolds; also a smaller copy of the same picture, in oils on panel (10″ × 8⅛″), without a history, but thought to be possibly the one by Cosway. (Both this picture and the last are in the Shakespeare Memorial Theatre, Stratford on-Avon.) We also learn from Mr Spielmann* that another version of the picture is in the possession of the Earl of Craven.

Whether the larger Stratford portrait could possibly be the one which was in the possession of Sir John Lister Kaye we do not know—the size is much the same—but it has not been possible to investigate the matter further.

The Wellcome portrait (Plate IX, Fig. 13) and the mezzotint (Plate X, Fig. 14), are here reproduced. They show differences in feature and expression which might give rise to doubts such as Wivell expressed concerning their common origin; but an engraver is not a photographer, and we have already seen how far from a facsimile an engraved copy—or a painted copy either—may be. But in the details of the hair, the collar, and the epaulettes upon the shoulder we find good evidence of relationship.

We now turn to the Wellcome portrait, 13″ × 11″, painted on canvas stretched over a panel. A comparison with the Stratford "Zoust" shows it to be another version of the same picture, and we also see correspondence with the details of the mezzotint. On the back of the picture are pasted fragmentary data which tally with the history of the picture which Malone identified as the original Zoust.

A mistake has been made, no doubt through the misreading of manuscript, in that Sir John Lister Kaye is here wrongly spelt Lister Raze, as also elsewhere† the name is mis-spelt Lister Raye. But that is a small point. With the help of

* *Encyclopaedia Brit.* under "Shakespeare's Portraits." An application to the Earl of Craven for information met with no reply.

† See page 22.

what is known of this portrait, and having regard to the fact that our completed lines must begin exactly underneath one another, we may reconstruct the original somewhat as follows*:

Wm. Shakespeare
*From a portrait by G. Zou*st from which a mezzotint was *scr*aped by Simon, *at one time in the posse*ssion of Wm. Douglas, Esq. of Teddington, near *Twickenham, who parted wit*h it to its present possessor, Sir John Lister Raze, *Bart., of The Grange, near Wak*efield, Yorkshire, for the sum of £400.

Possibly the opening words as we have restored them are not correct. That this was not the actual picture possessed by Wm. Douglas we know from Sotheby's measurements and our own; but that the two pictures are somehow related they themselves show. Beyond that we cannot go. As to the remainder of the restored portion, we have little doubt that it follows closely the missing original.

Thus the Wellcome portrait, purchased in Norwich, and bearing the name of Sir Thomas Browne written on a manuscript label pasted on the back, is identified as one of the various "Zoust Shakespeares."

Is it possible that we have by chance exposed an error two hundred years old? Are the Zoust Shakespeares perhaps all portraits of Sir Thomas Browne?

The original (whichever it may be) has long been discredited as a reliable presentment of Shakespeare: Zoust came too late to have painted Shakespeare from life, and if he intended to represent him at all, his portrait must either have been a fanciful one, or else a copy of some other (and there is no portrait of him known as yet which obviously served as Zoust's original).

But did he in fact attempt to portray Shakespeare? Our evidence consists solely of the statement made by Simon some forty odd years after Zoust's death.

We are far from making a definite charge of fraud if we suggest that the inscription beneath Simon's mezzotint may have been incorrect. Maybe the painting was not intended for Shakespeare; maybe it was not even by Zoust. The cheerful self-confidence with which people will assign a portrait to a certain artist, and name the person it represents—and affix labels accordingly, without finding it at all necessary to state whether what they record is a mere opinion or a historical fact—is unfortunately far too familiar to us. Possibly "T. Wright Covent Garden" was a sinner in this respect; possibly Simon himself was. Therefore we are quite prepared to believe that Zoust (if he was the artist) was not attempting Shakespeare at all, but was painting the portrait of a contemporary, its identity being forgotten later.

But it by no means follows that if this person was not Shakespeare, *therefore* it was Sir Thomas Browne: it might have been some other one of Zoust's con-

* As a possible help in identifying, at some future date, the catalogue (?) from which this fragment was taken, and thus establishing the history of the picture, the same type has been used as was used for printing the original lines.

temporaries between 1644 and 1681. And as the writer of the manuscript label on the picture has left no written justification of it as far as we know, he too may have been one of the class we have just had reason to condemn.

If we look for other evidence to help us to decide, we may think the fact that the picture was bought in Norwich suggestive*. It is; but not necessarily of this being Sir Thomas Browne. The very fact that a picture bore that label would make it more likely to gravitate to the city where his memory is held in such pious respect by so many admirers. Or if it were nameless, and in Norwich (possibly the fragment of printing was already there, but already only a fragment), and the owner wanted to identify it, Sir Thomas Browne's name would easily suggest itself, and he would be very pleased to detect in his picture a likeness to the portrait at St Peter Mancroft.

We are therefore thrown back on internal evidence alone, until other evidence is forthcoming.

If we were to select the Browne portrait which this most resembles, we should say the one at Norwich; of the Shakespeare portraits, the Chandos. Comparing it with these two, we think it more like the Browne than the Shakespeare in general appearance, though the high and rather perpendicular forehead is more reminiscent of Shakespeare.

All of Shakespeare's portraits represent him as bald over the forehead: the Wellcome portrait is not that, and it happens to show a little loose lock coming over the brow, as do so many of Sir Thomas's portraits. Neither the shape of this, however, nor anything else about the hair, suggests a close connection with any portrait of Sir Thomas Browne.

Again, the face we see in this picture is that of a comparatively young man†. It might have been painted *circa* 1650 when Sir Thomas Browne was 45 years old‡. Browne became famous at least eight years earlier, and the portrait cannot be discarded as a Browne portrait on the ground of age. It can only be on the ground of a lack of resemblance to other better authenticated portraits of probably a later date.

On this ground, we should hesitate very greatly to affirm, on the evidence, that the MS. label speaks the truth. But since we cannot absolutely prove that it does not, from the data so far considered, we must submit it, together with all others that are not clearly proved to be either copies of known portraits of Sir Thomas, or else portraits of some-one else, to the test of the skull.

* [It is worth noting, perhaps, that the Soest portraits of the Earles at Heydon Hall date from before 1667; and the forehead hair-curl and the general hair work of the Gunton portrait, suggest that the Norwich portrait may have been by Soest, and he might have painted Sir Thomas Browne more than once. The artist's name is given as 'Zoest' on the Heydon Hall portraits. ED.]

† [The Soest portrait of Erasmus Earle must have been painted when he was 60 years old and he looks to me not very much older than the Soest "Shakespeare." ED.]

‡ Horace Walpole's *Anecdotes of Painting*, under Gerard Soest, gives 1657 as the date of his earliest English painting, but this has now been thrown back to 1651.

(*i*) *The Sherborne Portrait* (Plate V, Fig. 7).

This portrait, bearing the name of "Sir Thomas Browne, M.D." inscribed upon its frame, forms part of the collection at Sherborne House, Northleach, Gloucestershire.

Mr John Lane, the publisher, was shown this picture by the late Lord Sherborne, who was not able to supply any details as to its history beyond those which are inscribed beneath the name, and which read: "Died 1682. Antiquary and Physician. Married a Daughter of Sir Ralph Dutton, Bart." Mr Milner assigns the picture to the period 1650—1665.

In dealing with the above statement, it will be well to set forth such facts as can be traced concerning the connection between Sir Thomas Browne and the Sherborne branch of the Dutton family. For the connection was certainly *not* the one stated here. Sir Thomas Browne married but once, and his wife was a Miss Dorothy Mileham, who survived him.

His Dutton connection was established at a much earlier period.

After the death of his father in 1613, his mother married again, her second husband being Sir Thomas Dutton, Knt. of Isleworth.

Further details concerning this gentleman are given in the Pedigree of the Garraway Family*, a document which we owe to the research of Mr R. Garraway Rice, F.S.A., barrister-at-law. The second husband of Anne Browne (born Garraway) is here entered as follows:

"Sir Thomas Dutton of Gloucester and Isleworth Co. Midds. Knt., son of John Dutton of Isleworth (son of Foulk Dutton of y^{e} Citty of Chester who died 11 April 8 Eliz.) by Catherine his wife (dau. of ..... Rowley), who re-married Raffe Wright of London, merchant. Bur. at Isleworth 19 May 1634 as 'S^{r} Thomas Dutton'."

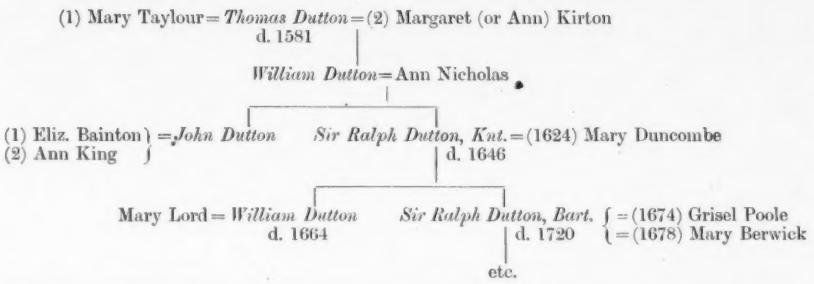
This carries us back to the grandfather of Sir Thomas Dutton, Foulk Dutton of Chester, who died on April 11th, 1566.

There were several Dutton families in Cheshire at this time, branches of the original Dutton stock. Ormerod in his *History of Cheshire* gives us pedigrees of the Duttons of Dutton, the Duttons of Hatton, and the Duttons of The Hey; and Fosbroke (*Hist. of County of Glos.* II. 388) gives the pedigree of the Sherborne branch of the Duttons and refers also to another branch, the Cloughton Duttons. In none of these pedigrees do we find any mention of Foulk Dutton of Chester. We cannot say, therefore, to which Dutton family he belonged.

Turning now to the Sherborne branch, we find that the first Dutton of Sherborne was a Thomas Dutton, a member of one of the Cheshire families, who purchased the Sherborne estate in 1551, and died in 1581. From him the successive owners of Sherborne were as follows†:

* Unpublished, except for the extracts given in this monograph, and in Chas. Williams' notes on "The Pedigree of Sir Thomas Browne."

† From Fosbroke (*loc. cit.*); and communicated by Lord Sherborne.


(1) Mary Taylour = Thomas Dutton = (2) Margaret (or Ann) Kirton
d. 1581
William Dutton = Ann Nicholas
(1) Eliz. Bainton
(2) Ann King
= John Dutton
Sir Ralph Dutton, Knt. = (1624) Mary Duncombe
d. 1646
Mary Lord = William Dutton
d. 1664
Sir Ralph Dutton, Bart.
d. 1720
= (1674) Grisel Poole
= (1678) Mary Berwick
etc.

Whether Foulk Dutton of Chester, who died 1566, and whose son migrated to Isleworth, was closely related to Thomas Dutton who settled at Sherborne in 1551 and founded a family there, we do not know. But even if so Foulk Dutton's grandson, Sir Thomas Dutton, Knt., of Isleworth, would be only slightly connected with the Sherborne Duttons. We might conjecture from the fact of Sir Thos. Dutton of Isleworth being also described as "of Gloucester," that intimacy between the two families might possibly have been promoted by their residence for some time in the same county; but it will be seen that suppositions as to any connection whatever are based on the slenderest foundations.

With the known facts in mind, let us examine afresh the inscription beneath the Sherborne portrait, which reads "Sir Thomas Browne, M.D.... Married a Daughter of Sir Ralph Dutton, Bart.," and see what mistakes have been crowded into this brief notice.

(1) Sir Thomas Browne did not marry a Miss Dutton but a Miss Mileham.

(2) Though connected with a Dutton family through his stepfather, this line was quite distinct from the Duttons of Sherborne.

(3) There is only one Sir Ralph Dutton, Bart., in the whole Sherborne line*. His parents were not married till 1624, and the date of his birth would therefore be later than this. This alleged father-in-law of Sir Thomas Browne did not himself marry until 1674, so that no daughter of his could have been more than 7 years old when Sir Thomas Browne died in 1682, aged 77!

It is obvious that whoever composed the inscription beneath the Sherborne portrait was so entirely ignorant of the facts of the case, that he did not even get Sir Thomas Browne into the appropriate generation. We are very much tempted to wonder whether he did not make a fourth mistake, in naming this a portrait of Sir Thomas Browne. Our misgivings are much increased when we compare this portrait with any of those whose claim to be a portrait of Sir Thomas is well authenticated. And we may find confirmation of our doubts in the considered opinion of the Director of the National Portrait Gallery. He has carefully examined the Sherborne portrait, and come to the conclusion that this is *not* a portrait of Sir Thomas Browne.

* In "Le Neve's *Knights*" (*Harleian Soc. Pub.* p. 267), the name of Sir Thomas Browne's step-father is given in error as Sir Ralf instead of Sir *Thomas* Dutton. Perhaps this helped the confusion.

We may be permitted to add here the remarks which have been prefaced by Lord Sherborne to the facts he has been good enough to supply concerning his family history. He writes: "I am not surprised to learn that it is not a portrait of that gentleman" (Sir Th. Browne), "nor do I know who imposed the inscription; but I suspect the author was the same individual who composed the inscriptions on the other pictures in this house, as they are full of mistakes, and the artists wrongly named in almost every instance."

(*j*) *The Devonshire House Group, generally known as the Dobson Group* (Plate VI, Fig. 8).

This group consists of a family of six people. The father standing behind to our left, three children in the forefront of the picture, and the mother seated to our right with the youngest child on her knee. It is in the possession of the Duke of Devonshire, and hung at Devonshire House in Piccadilly until the dismantling of the latter in 1921.

Tradition associates this picture with the Browne family, and it has been variously supposed (1) to represent Sir Thomas Browne as an infant on his mother's knee, with his father, sisters and brother; (2) to represent him as a man, with his wife and four children. We will examine the evidence for and against each of these alternatives in turn.

(1) A correspondent, "C. D.," writing to the editor of the *European Magazine* in 1801*, says that he finds a certain memorandum in the handwriting of Dr White Kennet (1660—1728), Bishop of Peterborough, in a copy of Sir Thomas Browne's *Works* (1686 edition) which formerly belonged to that prelate. The memorandum states that Elizabeth Littleton (daughter of Sir Thomas Browne) lent to the writer in Nov. 1712, a copy of the Rev. John Whitefoot's account of her father's life, and that in it was a note in Mrs Littleton's handwriting which he, Bishop Kennet, proceeds to copy. The note as it reaches us from Mrs Littleton *via* Bishop Kennet, "C. D.," and the *European Magazine*, includes the following statement concerning Sir Thomas Browne:

> "His picture is at the Duke of Devonshire's house in Piccadilly in his mother's lap. His father, mother, brother, and sisters in it. A family picture, his father being nearly related to that Countess of Devonshire whose picture is in the first room with her three sons by her, and very like to Sir Thomas Brown's father, as the servants shew to persons who go to see the picture, which is so good painting, that my Lord Duke values it at four hundred pounds."

This written memorandum of Mrs Littleton's, passed on to us through three transcriptions, is the authority upon which are based two statements frequently made: first, that we have in the so-called Dobson Group from Devonshire House a portrait of Sir Thomas Browne as a child; second, that he had two sisters and one brother. As far as I can discover, this memorandum is the sole authority for these statements. And indeed, unless there are any facts with which it is impossible to reconcile them, we should be inclined to consider the evidence good; though we might put in a caveat to the effect that a child having only two sisters and a

* *European Magazine*, Vol. XL. Aug. 1801, p. 89.

brother at the age of three to four (the age of the child in the picture), might possibly become possessed of more as time went on, and that it might be wiser in our historians not to limit the number definitely to three*.

Unfortunately the church of St Michael-le-Quern, Cheapside, was destroyed in the Great Fire, and with it its parish registers containing records of births and deaths in the family of Thomas Browne, mercer, of that parish, father of the future knight of the same name. Nor has any record as yet been found of the marriage of Thomas Browne and Anne Garraway. This record, if found, would tell us whether it was possible for a fourth child of their marriage to be born by October 19, 1605, the date of Sir Thomas's birth. Some facts are however known concerning the family, mainly through the researches of the late Mr Charles Williams of Norwich.

(i) Thomas Browne, the father, died in the year 1613, at some date between September 29th, when his will was made, and December 4th, when probate was granted.

His son Thomas would be 8 years old at this time.

(ii) He bequeathed the bulk of his wealth to "Anne my loving wife," and "Thomas Browne, Anne Browne, Jane Browne, and Mary Browne, the children of me the said Thomas Browne and Anne Browne my wife" and to another child not yet born.

Now, although the daughters were doubtless named in the order of their births, the will offers no evidence that Thomas was at that time the eldest of the family, since it was the custom then, as also now, to mention the sons before the daughters. Even if it had proved him eldest, the high rate of mortality among children which an examination of the Browne pedigree reveals† would quite prepare us for his being the youngest of four in 1608–9, and the eldest of four in 1613.

There is nothing therefore in any records that have as yet been traced, to conflict with Mrs Littleton's statement as to brothers and sisters. We now turn to the evidence of the picture. Again we have the advantage of Mr Milner's report on this Group-Portrait, after a very careful examination.

(i) The two eldest children on the left are undoubtedly girls and the two youngest on the right boys: this is shewn by the kind of dress worn, and by the fact, among others, that the girls wear pink and the boys blue, a tradition which survives with our babies today.

(ii) In the bottom right-hand corner, near the mother with the child in her lap, a skull is figured. This, he says, means either that the mother was already dead when this family group was painted; or that the child in her lap had died previously (the skull is placed equally near to both); or, less usually, that the skull was a general reminder of mortality.

It is obvious that if the skull conveys either of the first two meanings, the child in the mother's lap is not whom we have supposed, since Mrs Browne survived to become Lady Dutton, and Thomas survived to write *Religio Medici*.

(iii) The picture was painted in 1633 at the very earliest, and more probably not until 1640—50.

* See, among others, Mr Edmund Gosse's statement, p. 3 of *Sir Thomas Browne* (Macmillan & Co. 1905).

† "Pedigree of Sir Thomas Browne," by Chas. Williams, in *Norfolk Archaeology*, Vol. xv. 1904, pp. 109—113.

Then—it cannot have been painted in 1608–9. We are sorry; but the Director of the National Portrait Gallery is adamant. 1633 is the earliest possible date for the costumes; and though people may sometimes dress in a fashion long out of date, no family anticipates correctly the fashions of twenty-five years ahead.

We must conclude therefore that the Family Group tells us nothing of Sir Thomas as a little child. Hence it follows either that Elizabeth Littleton was misinformed, as people may easily be about family events of a century before; or that she was referring to some other picture at Devonshire House. Mr Francis Thompson, Librarian at Chatsworth, has been good enough to answer my questions on this point. There is no other picture in the possession of the Duke of Devonshire to which Mrs Littleton's description of the Browne Family-Portrait could apply; nor yet is there any group of a "Countess of Devonshire, her three sons by her," to whom the father in the first group was said to bear a resemblance.

In this connection it is worthy of note that Mrs Littleton's statement was handed on in manuscript by three different hands—her own, Bishop Kennet's, and "C. D.'s"—before it appeared in print: if a slight illegibility in any of these handwritings has led to a mis-reading of "three sons" for "two sons," the Countess in question might be the one in the disputed "Honthorst Group" whose history the Devonshire family has lost. Failing this, it is possible that both of the Group-Portraits existed as Mrs Littleton described them, but that both were lost in the fire at Devonshire House, to which the editor of the *European Magazine* refers in a note to Mrs Littleton's statement*.

(2) The other claim made for this portrait is that it represents Sir Thomas Browne with his wife and four children. This statement is derived from Horace Walpole, who, in giving a list of portraits by W. Dobson, says, "at Devonshire House is another family-piece of Sir Thomas Brown, author of Religio Medici, his wife, two sons and as many daughters†."

Here we have an identification of the persons in the group which certainly accords very well with the date prescribed by the costumes. A difficulty arises, however, from the fact that Dobson died in 1646, whereas Sir Thomas Browne's third child was not born until that year‡, he having married in 1641§. It seems therefore that the picture must either be not by Dobson, or not of Sir Thomas Browne and his family. It is suggested by Mr Collins Baker‖ that it may have been by Isaac Fuller, whose style much resembled Dobson's. But could it represent Sir Thomas Browne? The pedigree may help us.

From this we gather the following details as to the births of his children, and

* Mr Thompson has been unable to trace any reference to this fire in the family records; but as the Editor of the *European Magazine* referred to it in 1801, it must have been before that date.

† Vol. III. of his *Collected Works*, in four volumes, p. 237.

‡ "Pedigree of Sir Thomas Browne," by Chas. Williams, in *Norfolk Archaeology*, Vol. XV. 1904, pp. 109—113.

§ Memorial tablet in Church of St Peter Mancroft, Norwich. See Plate XIV.

‖ *Lely and the Stuart Portrait Painters*, Vol. I. p. 96.

we include the date of the death of all who did not live to the age of twenty-one years:

1644 Son
1645 Daughter
1646 Son
1647 Daughter
1648 Daughter
1649 Daughter; died 1652
1650 Daughter; died 1651
1652 Daughter
1655 Son; died 1662.
1656 Twin sons; died { 1656 / 1657 (aged 14 months)
1662 Daughter

Can we make any possible selection from these to correspond with the four children in the picture—the two eldest, girls; the two youngest, boys? To do so, we should have to date the picture 1657, making the youngest child an infant of not more than fourteen months, one of the twin sons born in 1656; and it is difficult to believe that the boy in his mother's lap was only just over a year old. We should also have to admit the improbable circumstance that Sir Thomas Browne left out two sons and two daughters from the family group. Further we should have to reconcile ourselves to the belief that the father of this family was the same man as we have seen appear already under different guises, but never so completely transformed as here*.

The effort is too much for us, and we find it easier to believe that if Horace Walpole is to be convicted of only one mistake in his mention of this picture, he was probably better informed in attributing the painting to Dobson, than in making Sir Thomas Browne and Family its subject. Or, of course, we could consign the picture he described to the same fire at Devonshire House as has already screened Mrs Littleton's reputation as a chronicler.

It would appear that a copy of this picture is somewhere in existence, for a photograph which was bequeathed by Mr Chas. Williams to Norwich Castle Museum shows this group with certain variations in detail which can hardly be accounted for by any "touching-up" of the photographic plate (see our Plate VI[bis]). This photograph is the same as is reproduced in the *Souvenir of Sir Thomas Browne* (1905). It differs from the picture in the possession of the Duke of Devonshire in several minor points. Thus the arrangement of the flowers in the hands of the

* It has been suggested that this family group, in some way associated by the traditions with Sir Thomas Browne, might represent Sir Thomas Browne's mother with her second husband, Sir Thomas Dutton, and their children. He died in 1634, so the picture could only have been painted, if its earliest date be 1633, thirty years after his wife's first marriage. It is highly improbable that she and he would then have any children of the ages in the picture! It is curious, however, that the men in both the Sherborne and Devonshire House portraits have a certain family resemblance, and both pictures have independently been linked with the Dutton family.

elder girl is different; the second girl has a row of buttons down the front of her dress instead of ribbon, etc.

There is an inscription on the back of the photograph, in Mr Williams' handwriting, which reads "Oil-painting by Vansomer in Devonshire House, Piccadilly." But this need not be taken as literally exact. Just as Mr Williams does not mention here that the painting is only *attributed* to Vansomer (to make the date square with the age of the supposed Sir Thomas) so he omits to state, what is apparently the case, that the photograph was not taken from the original picture, but only from some copy. We regret that we are not able to add the whereabouts of this copy.

(*k*) *Conclusions.* With the "Dobson Group" we complete our survey of the early engravings and of all the other portraits known to us which claim to reproduce the features of Sir Thomas Browne. The Sherborne, the Devonshire House, and probably the Wellcome, may be regarded as cases of mistaken identity; the 1669 engraving (5th edition, *Pseudodoxia*) as a deliberate imposture. The engravings by Van Hove and Van der Banck, and probably that by White; the Royal College of Physicians' and Bodleian paintings;—all these, together with the numerous reproductions to which they have given rise, may be put on one side as mere echoes of the miniature, some clearer, some more confused. The Gunton, again, reflects the Norwich. And from the crowd there emerge only the Norwich painting, the Buccleuch miniature, possibly the plumbago drawing, and the Wellcome portrait to submit themselves to the tests which the skull will supply.

(III) History of the Skull.

The history of the skull of Sir Thomas Browne is here recorded as it emerges after much disentangling of fact from rumour, and much testing of tradition by contemporary records*. Sir Thomas Browne died on October 19th, 1682, and five days later† his body was consigned to what was thought to be its last resting-place, a vault below the chancel of the church of St Peter Mancroft, Norwich.

Inscriptions in the chancel‡ commemorate the previous interment there of two of his grandsons, children of his daughter Anne and her husband Henry Fairfax. Both boys were named Barker Fairfax: one died in 1670, the other in 1673. Another inscription records the burial beneath the chancel of his daughter Mary, in 1676. In the absence of evidence it seems probable that the vault which afterwards received the remains of the great Physician had been built in 1670 or before, and held already the remains of these three descendants who had predeceased him.

* I here express my grateful appreciation to Dr Sydney Long, M.D., Physician to the Norfolk and Norwich Hospital, for his generous and valuable help in searching out these records, a help given unstintingly in other directions too, whenever it was asked; also to Mr Douro T. Potter, Sexton of St Peter Mancroft, for the information and assistance readily given in all matters relating to the church and its records.

† Burial recorded Oct. 24th, 1682, in church register.

‡ A complete list of the inscriptions in the church was given in 1723 by Mr Benjamin Mackerell, Author of the *History of King's Lynn*, in a MS. volume preserved in the British Museum, and entitled "The Monumental Inscriptions, Fenestral and other Arms, in the Parish Churches of the City of Norwich."

After a little over two years of widowhood Lady Browne died, on February 24th, 1685, and the vault was again opened that her coffin might be placed beside that of her husband. The mural inscription dedicated to her is the last of those relating to the family of Sir Thomas Browne in the church.

Our next information with regard to the resting-place of the distinguished Doctor comes from the pen of Mr Benjamin Mackerell, who copied the inscriptions etc. of this and other churches of Norwich, in the year 1723, and who, after recording that on the mural monument of Sir Thomas Browne, wrote the following*:

"The Late Dr. Jeffery gave notice to Sr. Thomas Brownes nearest Relative that his Vault wanted Reparation, who sent him word It might be filled up, which when the Dr. knew, desired that he might be Interred therin, who shortly after Dying His Corps was Deposited in the Vault & Afterwards filled up with Earth and over the Doctor on a Stone is this Ins.

M.S. Johannis Jeffery S.T.P. etc.....
Aprilis Ærae Xnae 1720, Ætatis suae 73."

The Dr Jeffery to whom reference is here made was appointed "minister" of St Peter Mancroft four years before the death of Sir Thomas Browne; and to him in 1716 was confided the task of editing *Christian Morals*, when this missing manuscript was at last discovered.

Thus to the remains of Sir Thomas Browne, of Lady Dorothy Browne, and probably of their daughter and two little grandsons, were added those of his friend and admirer, Dr John Jeffery. The vault which enclosed them was then filled in with earth†. A period of 120 years now elapsed. During this time the lead of which Sir Thomas Browne's coffin was made gradually became corroded, until at last it was "completely decomposed and changed into a carbonate which crumbled at the touch‡." The accidental "touch" which broke open this now fragile shell was given in the year 1840. In August of this year, excavation beneath the chancel floor proceeded afresh, in order to prepare a new vault for the reception of the body of Mrs Bowman, wife of the then incumbent of St Peter Mancroft. In the course of these operations the workmen dug right into the earth which had been carried there in 1720; a blow of their pickaxes struck and pierced a coffin concealed by the soil; and an examination of the brass coffin-plate revealed the fact that the human remains which were thus exposed were those of their most famous citizen.

The date of Mrs Bowman's death was Saturday, August 8th, 1840. Operations in the chancel would not be begun before the following Monday, August 10th, and we may take this day, or the next, as the one on which the discovery was made. Norwich at that time boasted two newspapers, weeklies. By Saturday, August 29th, the following item of news found its way into the *Norwich Mercury*:

"While the persons employed in opening the ground for the interment of the late Mrs Bowman were at their work, they accidentally struck a coffin and exposed the corpse, which proved to be that of the great Sir Thomas Browne. The features it is told us were perfect, and especially

* MS. in Brit. Mus. *loc. cit.*

† This is a step frequently taken in the case of vaults which are no longer in condition to support safely the church paving above them.

‡ See letter p. 35.

the beard. Sir Thomas was accounted one of the handsomest men of his time" (the journalistic touch!). "The circumstance becoming known the men deemed it best to cover the remains immediately."

It is evident from the last sentence that the news became somewhat bruited about, and that the attention and visitors attracted were such as to make the workmen uneasy, so that they deemed it best to cover from view what they had found. Persons in authority in the church however, such as the sexton (at that time "lord of the dead"), the churchwardens (the responsible officials in any alterations to the fabric), or any to whom they might give permission, would still be able to examine the remains, as long as they were accessible. This we may take to be until Friday, August 14th, at latest: on that day Mrs Bowman was buried, and the work of sealing the vault and replacing the chancel floor would be completed the same day, in order to allow time for the cleaning of the church in readiness for the following Sunday. Thus, we gather that for four days at most—August 10th to August 14th—and maybe for less, it was possible to have access to the coffin, and that during that time others, besides the workmen, viewed its contents.

The writer of the newspaper paragraph quoted above does not claim himself to have been among the eye-witnesses, and in the following number of the *Mercury* (Saturday, September 5th) his account is corrected by one who actually saw the remains. This is a Mr John Wortley of Norwich, who sends the following letter to the Editor:

"Observing in your paper of last week a paragraph stating that the remains of Sir Thomas Browne were found in a state of great preservation, I beg leave to correct the report given by your correspondent, that the head and features were perfect,—such was not the case, nothing more was found than in ordinary instances, the bones being perfectly bare; the skull was of the finest conformation, the forehead being beautifully developed."

The inaccuracies with which this second description closes evoked a letter from another eye-witness in the next issue of the *Mercury* (Saturday, September 12th):

"Sir, Allow me to correct the errors of your last week's correspondent with respect to Sir Thomas Browne's relics. It is true that no trace of the 'features' remained, but the 'beard' was in good preservation, and of a fine auburn colour; the forehead was remarkably small and depressed; the head unusually long."

This correspondent was a Mr Robert Fitch, of whom more anon. His version in the *Mercury* remained uncontradicted.

On the same date, September 12th, there appeared a more detailed account in the *Mercury's* contemporary, the *Norfolk Chronicle*. It found a place in the editorial news-column, as an item of news of which "we are informed." The account is as follows:

"On the occasion of an interment which recently took place in the chancel of St Peter Mancroft Church, in this city—the workmen employed to prepare the new vault, broke with a blow of their pick-axe, the lid of a coffin, which proved to be that of one, whose birth and residence within its walls conferred honour on Norwich of the olden time—the once celebrated and still by his writings well-known Sir Thomas Browne. This accidental circumstance afforded an opportunity of inspecting the remains. The bones of the skeleton were found to be in good preservation, particularly those of the skull. The brain was considerable in quantity but changed

to a state of *adipocere*—resembling ointment of a dark brown hue. The hair of the beard remained profuse and perfect though the flesh of the face as well as of every other part, was totally gone. With respect to the conformation of the head, we are informed that the forehead was remarkably low, but the back of the cranium exhibited an unusual degree of capaciousness.—The coffin-plate was of brass, in the form of a shield, and bore the following inscription:

AMPLISSIMUS VIR
DNS. THOMAS BROWNE, MILES, MEDICINAE
DR. ANNOS NATUS 77 DENATUS 12* DIE
MENSIS OCTOBRIS, ANNO DNJ. 1682. HOC
LOCULO INDORMIENS, CORPORIS SPAGY-
RICI PULVERE PLUMBUM IN AURUM
CONVERTIT.

For the accuracy, in every respect, of the above copy we can safely vouch, since it is carefully taken from an actual impression of the engraving on the plate. Not so, however, as to the 'doing into English,' which is here subjoined merely for the use of 'readers in general.'——Whether the last two lines of the original latinity were meant to predict an alchemic transmutation, or to express a hyperbolical compliment, we leave to the learned, with this remark that the coffin is *still a leaden one.*" (Here follows a translation of the inscription.)

Thus the account in the *Norfolk Chronicle* of September 12th. Again the Editor "is informed": no name is given. It is interesting, however, to compare the wording of this account with that contained in a letter sent on December 4th of the same year to the Treasurer of the Society of Antiquaries† (London), bearing the signature "Robert Fitch." This letter forms part of a volume of MS. letters bequeathed by the late Mr Charles Williams to Norwich Castle Museum, and runs as follows:

"Norwich, December 4th, 1840.

Dear Sir,

Feeling confident that anything connected with the history of one of the greatest men of his age will be interesting to the Fellows of the Antiquarian Society, I send you a short account of the discovery of the remains of the celebrated Sir Thomas Browne in the church of St Peter Mancroft, in this city. . . . . n‡ who were employed in digging a vault in the church, accidentally broke with a blow of their pick-axes the lid of a coffin which proved to be that of one whose residence within its walls conferred honour on Norwich of the olden times. This circumstance afforded an opportunity of inspecting the remains. The bones of the skeleton were found to be in good preservation, particularly those of the skull. The forehead was remarkably low and depressed, the head unusually long; the back part exhibiting an uncommon appearance of depth and capaciousness. The brain was considerable in quantity, quite brown and unctuous. The hair profuse and perfect of a fine auburn colour. The coffin-plate which was also broken was of brass, in the form of an antique shield, and bore the following inscription:" (here follows the inscription, with the date of death given rightly as October 19th) "I succeeded in taking a few impressions from the plate, will you present one to the Antiquarian Society?

There was another singular circumstance connected with the discovery. The lead of which the coffin was made was completely decomposed and changed to a carbonate which crumbled

* The day of the month was here mis-read from the plate as twelve. The "19" of the inscription is followed by a dot, which makes it look somewhat like "12". See reproduction of plate in Mr Chas. Williams' *Souvenir of Sir Thomas Browne.*

† The letter was read at a meeting of the Society on December 10th following, and an abstract of its contents is given in the *Gentleman's Magazine*, Vol. xv. New Series, 1841.

‡ Words obliterated in the MS.

at the touch. Now as the substratum is chalk (carbonate of lime) would the carbonic acid in the chalk cause in so short a time as 160 years the complete decomposition? but this is not an antiquarian question.

I have the honour to be
Dr. Sir
Yours faithfully
ROBERT FITCH

Thomas Amyot Esq.
Treasurer of the Antiquarian Society"

If we compare this letter of December 4th with the account in the *Chronicle* of September 12th we must conclude either (1) that Mr Fitch in phrasing his letter made use of the version sent to that paper by another witness, whose testimony agreed with his own given shortly on the same date in the *Mercury*, and who, like himself, had rubbings of the plate, or (2) that Mr Fitch, contenting himself with a brief correction in the *Mercury* of the mistakes of its correspondent, reserved the more detailed communication for the paper he preferred. Either alternative would serve, but the second seems more probable, especially if we suspect that Mr Fitch had reasons for not wishing his name to figure too prominently in local discussions of the affair, and requested the Editor of the *Chronicle* not to refer to the source of his "information."

We have then here (on September 12th and December 4th) three entirely consistent accounts of the finding and contents of the coffin of Sir Thomas Browne, two of which appeared in Norwich papers within a month of its having been inspected by several other persons. The fact that these remained uncontradicted is worthy of notice. It gives us, however, only negative evidence of their accuracy, and what is more important for appraising the value of these accounts, is an enquiry into the character and reputation of the witness who furnishes us with two of them, and probably also with the third.

Our questions concerning Mr Fitch find an answer in the following excerpt from Charles Mackie's *Norfolk Annals**:

"Mr Fitch.....was a partner in the firm of Fitch and Chambers, chemists and druggists, Norwich. In 1858 he was appointed upon the commission of the peace, and in 1867 became Sheriff of Norwich. He was connected with the directorate of several public companies, and for more than thirty years was an honorary secretary of the Norfolk and Norwich Archaeological Society. Among his literary contributions to the Society's proceedings was an admirable description of 'The Gates of Norwich.' Mr Fitch was widely known as a collector, and possessed one of the finest private collections in the kingdom, which some years before his death he handed over to the Trustees of the Norfolk and Norwich Museum."

We may add that he was a Fellow of the Geological Society, and of the Society of Antiquaries. Also that he attended the church which held the remains of Sir Thomas Browne disturbed in 1840, and was appointed to the office of churchwarden there five years later, in 1845.

If one reflects on the characteristics that are implied by this record, one concludes that ability, observation, capacity for detail, a mind alive to many interests and energy in the pursuit of these were prominent among them.

* Vol. II. p. 459, pub. 1901.

An incident that must here be recorded throws another side-light upon his character. It will be remembered that in his letter to the Treasurer of the Society of Antiquaries*, Mr Fitch mentions having taken a few impressions from the coffin-plate, though without implying that he had removed the plate in order to do so. It evidently became known in Norwich (how, it is not essential to the present enquiry to discover) that the plate had been taken from the coffin; and it was suspected that it was in Mr Fitch's possession. Probably this suspicion was voiced on many occasions. Of one occasion towards the end of his life, however, we have a definite record. The present sexton of St Peter Mancroft was appointed to the position in the year 1893, two years before the death of Mr Fitch, and he makes the following statement, a statement confirmed by contemporary records†:

"It was known that the late Mr Robert Fitch possessed some paper impressions from the plate which belonged to the coffin of Sir Thomas Browne. He (Mr Fitch) was questioned by the late Mr Charles Williams as to what became of the plate after the impressions were taken. He said it was returned to the sexton who probably put it away in one of the church chests. The late Rev. W. Pelham Burn, who was then vicar, ordered me to search for it in the chests and drawers. I made search but could not find it. Mr Fitch died on April 4th, 1895, and soon after his death the plate was found in a 'secret well' of a desk that had been used by him. The late Mr John Fitch, his son, returned the plate to the vicar in the following June, when it was placed in a case in the sacristy where it remains today. Some time after the return of the plate, Mr Charles Williams was collecting matter for a pamphlet he was writing about Sir Thomas Browne, and while I was helping him to search the church records I asked him if the story I had heard about Mr Fitch and the coffin-plate, and which I have just related, was true. His answer was 'Yes'."

We gather therefore, that Mr Fitch's antiquarian zeal sometimes led him to take steps which he was not afterwards prepared to admit, and even went so far as to deny. This incident may lead us to question the entire suitability of Mr Fitch for the office of churchwarden, but does nothing to call the value of his evidence as antiquarian in question. That he was a keen antiquarian his works do testify, and where the two interests clashed, we see it was not the latter that gave way.

Thus we feel we may accept without misgiving the accounts given on September 12th and December 4th, 1840, and rely upon their description of the contents of the coffin and of the human remains which it enclosed. They also give a detailed description of a skull whose peculiarities are sufficiently well marked for a verbal picture to serve to distinguish it from most other skulls.

We turn now to another contemporary record which has been handed down to posterity: one which figures for us the skull discovered and handled in 1840 with more exactness than the most minute verbal description could convey. This document consists of a plaster-cast, in the keeping of the Norwich Castle Museum, and entered in the catalogue there as "Cast in plaster from the skull of Sir Thomas Browne presented by Mr Charles Muskett on October 25th, 1841."

* See p. 35.

† *Notes and Queries*, 8th series, Vol. VI. Sept. 22 and Oct. 6, 1894, and Vol. VIII. Oct. 1895, p. 325.

We may confirm the details given in this entry by reference to the next Museum Report* after this date, viz. the report of 1842.

The same report acknowledges the presentation of a "Phrenological Collection" of skull-casts by another donor; and it is evident, from the omission of the facial portion and most of the base in Sir Thomas Browne's cast, that we owe this plastic record to the keen interest which phrenology excited at that time. A lecture on phrenology had in fact been given in Norwich a few months before the finding of the skull and had aroused much discussion†.

But who was Mr Charles Muskett? Dr Long has kindly furnished me with the answer to this query:—Charles Muskett was a well-known Norwich printer and publisher whose place of business was no. 5 the Old Haymarket, just opposite the church of St Peter Mancroft. He had antiquarian interests; he published, and issued from his own printing-press in 1850, a large illustrated folio volume entitled *Notices and Illustrations of the Costumes, Processions, Pageantry, etc. formerly displayed by the Corporation of Norwich.* He, like Mr Fitch, was a prominent parishioner at St Peter Mancroft and held the office of churchwarden there from Easter 1848 to Easter 1850. We shall probably be correct in surmising that he was one of the persons who inspected the remains *in situ* when the accident to the coffin became known. It seems also very likely that he was a friend of Mr Fitch: certainly they must have been closely acquainted‡.

Whether Mr Muskett himself made the cast presented in 1841 we do not know. It is obvious that for the cast to be made the skull must have been taken from the grave; by whom, and whether by permission of the workmen, sexton, or churchwardens (or actually by one of these), again we do not know. There are hearsay accounts which attempt to fill in this gap in our information, but we will reserve those till later, and deal here with our store of actually ascertainable facts. There is no record in the Diocesan Faculty Book of alterations to the church in 1840–1; there are no Church inscriptions, and no entries in the parish register, relating to other burials in the chancel during that time; "moonlighting," when once Mrs Bowman's vault was sealed, and the very heavy marble slabs were fitted in above it, would require more time and assistants than would be safe. So the skull must have been taken during the preparation of Mrs Bowman's vault.

Possibly the intention was to return it; quite probably it was only "borrowed," for casting. But the utmost limit of time during which it was accessible was four days and probably less; out of that we have to allow for the news to be spread about, and attract an inconvenient number of visitors; and the time still remaining in which the skull could be taken, cleaned, cast, and returned was all too likely to prove inadequate. This, concerning the possible intention to return it, is mere speculation. But the salient fact remains that Sir Thomas Browne's skull was taken from the coffin between August 10th and August 14th, 1840.

* In 1841 this Museum was not housed in Norwich Castle as now, and was called the "Norfolk and Norwich Museum."

† See Mackie's *Norfolk Annals*, Vol. I. p. 393 (Extract from the *Norfolk Chronicle*, Feb. 1840).

‡ His name appears in the 1850 list of members of the Norfolk and Norwich Archaeological Society (founded 1846) and he remained a member till his death about six years later.

To this fact we may add another: it was never returned. The skull which has been preserved as that of Sir Thomas Browne for more than seventy years in the Norfolk and Norwich Hospital Museum answers to every point of the description left on record by Mr Fitch and we identify it as the original of the cast given by Mr Muskett*.

The history of its wanderings from the grave to the hospital is incomplete; nor can we date its arrival more exactly than by saying it was before October 19th, 1848. This date is furnished from the Hospital Museum records by Dr Long, who writes as follows:

"The first separate pathological Museum that we had at the Norfolk and Norwich Hospital was built in 1845, and formally opened on the 10th of September of that year. Previously the pathological specimens had been arranged in galleries round the operating theatre. Mr Donald Dalrymple was then Hon. Curator of the Museum, having been appointed on December 3rd, 1844. At a meeting of the Museum committee on October 19th, 1848, 'The Secretary (Mr D. Dalrymple) announced that since the last meeting (June 29th, 1848) a complete catalogue of all the contents of the Museum had been made, that every preparation had been classed, numbered, described, and labelled under his directions, and that the work had been most thoroughly performed by Mr Harper, the sub-curator †.'

Mr Dalrymple's catalogue is still in the possession of the Hospital Museum; and item no. 641, N. 5 is therein described as 'Cranium of the celebrated Sir Thomas Browne.' By the side of this entry, in the column headed 'Whence derived,' is written 'Dr Lubbock.' The handwriting is the same as for all the other items."

Thus it is clear that the skull was in the possession of the Hospital Museum by October 19th, 1848, though the Museum has no record to show how long it had been there. In 1894 Mr Charles Williams and Mr James Hooper‡ stated variously§ (1) that Dr Lubbock presented it in 1845, (2) that it was handed to the Museum Authorities after his death in 1847‖.

One circumstance may be mentioned here which tends to support the second of these alternatives. In July–August 1847 the Archaeological Institute of Great Britain and Ireland held its meetings in Norwich. It will be remembered that

* For the detailed comparison between the Norwich Castle Museum cast and the skull preserved, see later, pp. 45–51.

† Extract from Minute-Book of the Museum Committee of the Norfolk and Norwich Hospital.

‡ Mr James Hooper was a journalist and writer on local antiquarian subjects. Both he and Mr Williams have died within the last few years, and neither was of an age to recall the events of 1840–8, though they might have got first-hand evidence from older men still living in 1894. The Rev. Chas. Turner uses the word "presented" in a letter written in 1885 (see p. 43).

§ See *Notes and Queries*, 8th series, Vol. VI. Sept. 22 and Oct. 6, 1894.

‖ Mr Hooper adds that a lock of Sir Thomas' hair was given with the skull. As to this, Dr Long informs me that a lock of hair *was* preserved with the skull in the Hospital Museum, though it is unfortunately now missing. This lock was however presented by Mr Chas. Williams in 1888, as he himself states (*Catalogue of the Portraits in the Board Room of the Norfolk and Norwich Hospital*, pub. 1890). Nothing has been found to show how Mr Williams obtained it, and nothing as to its earlier history. Dr Long, however, confirmed the statement that the hair was auburn in colour. There was also a lock of hair, said to be Sir Thomas Browne's, in the possession of the late Sir William Osler; Lady Osler states that this was obtained from a Mrs Wilkin of Norwich, and, as it was not specifically mentioned in Sir William's will, had to be sold with a large number of his books, at the order of his Executors. It was bought by Messrs Tregaskis, Booksellers of London, and has now been sold by them to Mr Sherman, an American professor.

Mr Fitch was closely associated with the local Archaeological Association*, and on this occasion he repeated for the benefit of their visitors the account he had already given in 1840 of the finding of Sir Thomas Browne's coffin, using much the same words as before†.

His account is reported in the *Proceedings of the Archaeological Institute* for that year‡. To "the hair profuse and perfect, of a fine auburn colour" are added in this report the words "similar to that in the portrait presented to the parish by Dr Howman, and exhibited at the meeting of the Institute in 1847, and which is carefully preserved in the vestry of St Peter Mancroft." It seems probable that if the skull had been exhibited, as well as the picture, this fact would also have been mentioned. It seems likely, too, that if the skull were already on show in the Hospital Museum, it too would have been borrowed by someone for this occasion, or at least referred to, to illustrate the description given.

Dr Lubbock died on March 4th, 1847, and his affairs would not be settled and his effects distributed until about six months or more from that date. If, as Mr Hooper stated, the skull was given to the Hospital Museum after his death, it would not have been handed over by July 29th when the Archaeological Institute met in Norwich, nor would he himself be alive to lend it, if he so wished.

We cannot, however, presume too far on the assumption that if the skull had been in the Hospital Museum by August 1847, it would have been exhibited, because we know that the skull-cast had been in the Norfolk and Norwich Museum since 1841, and that it also was not referred to with a view to illustrating "the forehead remarkably low and depressed" etc. Still, the skull-cast was not likely to have attracted so much attention as the skull, nor the fact of its existence in a public Museum to be so widely known: those who had most to do with its presence there were perhaps unwilling to call attention to it, and apparently no one else produced it for the edification of the visiting society.

Summary. We will now resume briefly the facts that may be considered established in the history of the skull we have been considering:

The coffin was broken into during excavations on August 10th or 11th, 1840.

The coffin-plate, the skull and some locks of hair were taken from the coffin at some time between August 10th and August 14th.

In the course of the month following, reports of the discovery appeared in the local press from three, or possibly four, different sources.

One of the observers was Mr Fitch, an antiquarian of some ability, who gave a description of the remains which he amplified a few weeks later in a letter to the

* He was one of the original members enrolled at the founding of the Society in 1846. He was both Hon. Treasurer and Hon. Secretary for about 30 years, resigning from the former post in 1887, and from the latter a year later.

† See p. 35.

‡ See "Note on the discovery of the remains of Sir Thomas Browne in 1840," by Robert Fitch, F.G.S., in *Proc. Archaeol. Inst.* 1847, p. 223.

Society of Antiquaries. This included a careful description of the singular characters of the skull, and was accompanied by rubbings of the coffin-plate.

On October 25th, 1841, a plaster-cast of the skull was presented to the Norfolk and Norwich Museum (now Norwich Castle Museum).

At some date before October 19th, 1848, and probably during the year 1847, the Norfolk and Norwich Hospital became possessed of a skull, which is entered in their earliest catalogue as "The skull of Sir Thomas Browne," and which corresponds both with the description published within four weeks of the discovery in 1840, and with the plaster-cast taken probably at the same time, and certainly before October 25th, 1841.

A lock of hair,—of the colour observed at the time of discovery—which was preserved with the skull for some years from 1888 onwards, is now missing. A lock, said also to be of Sir Thomas Browne's hair, is now in America.

The coffin-plate, also removed in 1840, was restored to the church of St Peter Mancroft in 1895, and is now in the sacristy. The skull itself, preserved in the Museum of the Norfolk and Norwich Hospital since before October 1848, was re-interred beneath the chancel of St Peter Mancroft on July 4th, 1922.

Objections considered. Our researches have fully justified the tradition carried down through two generations that here was the skull of the great Sir Thomas Browne. And it is instructive to see what type of objection has been raised here and there against this tradition since the generation that remembered passed away, and the evidence they left was lost sight of by their successors.

It has been objected* that this cannot have been the skull of Sir Thomas Browne, because it is obviously the skull of a woman. Now it is a fact well known to every craniological student that skulls vary very considerably in the degree in which they manifest the characteristic sexual traits. Some may be classified at once and without doubt as male, or as female, while others may give rise to difference of opinion. It is in the first of these two groups that we put the skull in question: this is one that has strongly marked *male* characteristics. The prominent supra-orbital ridges, the well-defined inion, the irregular occipital surface with its well-marked ridges for muscular attachments, the robust mastoids—these and other features leave little room for doubt as to the sex.

Again we are cautioned that "an American surgeon," on being shown the skull of Sir Thomas Browne, "laughed heartily and replied that for his part he should class it as that of a Peruvian!" Alas, Sir Thomas—who, if thou didst jest by accident wouldst "blush at the levity of it"—that thou shouldst be made the subject of this transatlantic joke! For that such it is, we might be able to convince the correspondent who reports it so seriously, if he would come and examine here at the Royal College of Surgeons a series of the artificially-deformed crania referred to. The difficulty, however, of associating the low forehead of Sir Thomas Browne with the high intellectual capacity he evidences, has been felt by others than the

* Letter to Sir Arthur Keith.

American surgeon. I suggest that it is their previously-conceived ideas on this subject which need revision: a high brow does not invariably denote intelligence nor is it a necessary condition of the same.

Another difficulty has been suggested by the references to "the hair profuse and perfect, of a fine auburn colour." It has been hinted that this description by Mr Fitch is so unlikely to be correct (Sir Thomas being 77 at the time of his death) that it casts a doubt upon the value of any of Mr Fitch's evidence*. The alternative suggestion, which Mr Williams also made†, is that Sir Thomas Browne wore, and was buried in, a wig. I do not think we can adopt either of these suggestions. As regards the latter, Sir Thomas gives his views on wigs in a letter to his son Thomas in November 1664:—"Be sober and complacent, *If you would quit periwigs it would be better and more for your credit.*" This was eighteen years before his death; but since we know from the Rev. John Whitefoot's much-quoted description that no change of mode induced him to abandon his sober and old-fashioned cloak, we can be reasonably certain that he also maintained to the end his prejudice against wigs. As to the other theory, that Mr Fitch reported incorrectly: the difficulty about the colour is removed by the fact that hair of persons long buried is commonly found to have acquired a reddish tinge, whatever the original shade. Again, as to the "profusion" of the hair‡: it is not impossible, even in these degenerate days, to find a septuagenarian of whom it is quite untrue to say that:

> ".....years of sorrow and of care
> Have made his head come through his hair."

In this connection, however, it should be pointed out that the earlier accounts§ only refer to the "hair of the beard," and perhaps Mr Fitch on December 4th‖ had this more in mind than the hair of the head, although he there omits to particularize.

One final argument against us shall be stated here with the same candour as has been observed hitherto. It comes from a gentleman who remembers a friend now deceased telling him of an experience made during alterations to the chancel of St Peter Mancroft at the end of the last century. The friend tested one of the lead coffins in a vault—not Sir Thomas Browne's coffin, but a coffin—and found it to be composed of lead an inch thick. Therefore the labour of extracting Sir Thomas Browne's skull would be impossibly great under the conditions obtaining. Having calculated that a coffin made of lead an inch thick would weigh about half a ton, and estimated at nil the chances of its having been used, we refuse to be daunted in our belief by this narrative. Lead coffins, moreover, do crack, corrode, and crumble, and the experience of the present sexton, Mr Douro Potter, furnishes many examples

* *Brit. Med. Journ.* May 6, 1922.

† *Notes and Queries*, Oct. 6, 1894.

‡ [The portraits taken possibly about ten years before his death show a good profusion of hair. The words used in the *Proceedings of the Archaeological Institute* (see our p. 40) suggest that the colour of the hair in the St Peter Mancroft portrait was originally light or at least of a visible colour, and this would correspond with the Gunton copy. Ed.]

§ See pp. 34 and 35.

‖ See p. 35.

similar to the one described in the account of the finding of Sir Thomas Browne's remains.

A Disputed Item. The imperfect history of the skull has now been given, and difficulties in the way of accepting it dealt with. I have reserved till this point an account which attempts to fill in the gap in the story, and may indeed do so correctly. But that, I think, is not proven.

The account deals with how the skull found its way from the coffin to the collection of Dr Lubbock. It has been repeated on various occasions by the late Mr Charles Williams, echoed by later biographers, and hotly resented by the descendants of the sexton whom the story implicates. It runs as follows: "The skull was abstracted by the sexton, one George Potter, by whom it was offered to the late Mr G. W. W. Firth, one of the surgeons at the hospital. On his refusing to purchase it, the late Dr Edward Lubbock became its possessor*."

Mr Edmund Gosse improved upon this story, in 1905, with the following dramatic narrative:

"(The workmen) broke into the neighbouring vault where the body of Sir Thomas Browne had lain since 1682, and called the sexton to look at what they had found. The sexton saw below him the skeleton of the Doctor, and bending into the tomb, seized the skull and carried it off. He offered it for sale, and it was bought by a collector over whose name, in my opinion, it is best to shed the poppy of oblivion†."

We feel we must apologise to Mr Gosse for rudely disturbing, in the course of our enquiry, both the slumber of the collector's name, and the dream which spreads forth such an entertaining picture...the broken coffin, the workmen's call, and ha! the sexton's hawk-like swoop and sudden exit with his prey! Alas,—but truth demanded it.

We turn back to the account as given by Mr Williams. And our contribution to this much-disputed subject will consist in pointing out the source from which Mr Williams derived his information, and in furnishing the reader with any data we can find which may help him to form an opinion upon it. As to the source: there is in the book of MS. letters bequeathed by Mr Charles Williams to the Norwich Castle Museum one written in 1885 by the Rev. Charles Turner, vicar of St Peter Mancroft from 1848 to 1879. There is nothing in the letter to show to whom it was addressed, but it replies to an enquiry which was probably put by Mr Williams himself. The following is a copy of the letter:

"Framingham Earl,
Norwich.
14th Oct. 1885.

Dear Sir

I cannot speak with authority about the skull of Sir T. Browne but my memory says that Potter the sexton took the skull from the coffin at the time of the funeral of Mrs Bowman, the wife of the then incumbent of St Peter's; he offered it for sale first to Mr Firth who declined; then to Dr Lubbock who bought it and presented it to the hospital. In the above transaction *a skull* was bought, it might have been that of Sir T. Browne or any other person,

* *Notes and Queries*, Oct. 6, 1894.
† *Sir Thomas Browne*, by Edmund Gosse, p. 116.

about this I know nothing. Potter's children are living in St Peter's Parish and one of them is sexton thereof. Mr Fitch has a rubbing facsimile of the brass plate on the coffin.

I remain,
Yours very truly,
CHAS. TURNER."

The first thing to be ascertained in connection with this account, was whether the Rev. Chas. Turner was intimately connected with Norwich and cognisant of current happenings there, before his appointment to St Peter's in 1848. We are indebted to the Rev. A. E. Alston (nephew of the Rev. Chas. Turner's second wife) for information on this subject. Mr Charles Turner came of a Norwich family; his father was twice Mayor of Norwich. He was Perpetual Curate at Cringleford (two miles from Norwich) from 1835 to 1849, and at the same time was vicar of the Church of St Michael-at-Thorn, Norwich.

There is no doubt, therefore, about Mr Turner's close connection with Norwich before 1848, as also after. With regard to his letter, we can only offer the following observations: Mr Turner was speaking of events which had happened 45 years previously. On the other hand, we must remember that an old man's recollection of the events of his early prime is apt to be very clear.

Again in spite of the note of uncertainty with which his letter opens, he does seem to be speaking decidedly and definitely of things he clearly remembered. What he states is, we should say, quite probably the same as what he understood to be the case at the time. How well informed he was then, we have no means of knowing. We only note that he does not mention that the skull was used for casting, nor its having been in the hands of anyone to whom we have reason to attribute the cast. And that he himself protests that he cannot speak with authority on the subject.

It seems on the face of it that men of Mr Fitch's or Mr Muskett's antiquarian and scientific leanings would have more interest in "borrowing" the skull than the sexton. If it were borrowed by permission of those in charge, these gentlemen, as prominent parishioners and "savants," would be best able to secure such permission. If *without* permission, "moonlighting" would not be so difficult a matter while the vault of Mrs Bowman was still open. Whether, when it had been used, it was returned to the sexton or not to dispose of, we cannot say. But, remembering the incident of the coffin-plate, we should not find it out of the question to believe that Mr Fitch let it be understood it had been given to the sexton when such was not the case. And we recall that when the Rev. Charles Turner came to St Peter's Mr Muskett was churchwarden for the first two years, and closely in touch with him, and Mr Fitch was ex-churchwarden.

All these are factors to be considered and it seems fair to sum up as follows: The Rev. Charles Turner's letter provides a strong argument in favour of the story cited, but it is by no means conclusive. The story will doubtless be repeated, but to our view it should not be repeated as an historical fact.

Can we not now say to this controversy, as to the illustrious relic which occasioned it?—

REQUIESCAT IN PACE!

(IV) COMPARISON BETWEEN THE 1840–1 CAST AND SOME OF THE 1922 RECORDS.

(*a*) *Description of the* 1840–1 *Cast.*

The cast presented to the Norfolk and Norwich Museum in October 1841 as that of the skull of Sir Thomas Browne is a very amateurish piece of work. The several pieces in which it was made have not been put together to fit exactly. On the left side (see Plate XV) a seam remains which runs backwards from a knot of plaster close behind the lateral projection of the frontal bone, and then turns downwards towards the auditory meatus. The section which it marks off is nowhere brought quite level with the adjacent portions of the skull-cast, and in the neighbourhood of the zygomatic ridge the amount of depression attains its maximum of one millimetre. A somewhat similar seam occurs on the right side of the cast: here the section is depressed about half a millimetre until the seam approaches within a centimetre's distance of the zygomatic arch. At this point a dab of plaster appears to have been put on with the idea of raising the level, but with the result of raising it too much. Another seam, which may be plainly seen in Plate XVI, runs from mastoid to mastoid above the inion. This seam in its lower parts is merely a ridge dividing two surfaces fairly evenly matched together; in its upper part, however, where it passes round the inionic region, it reveals the fact that once more the section, when being fitted on, was pushed in slightly too far, this time about half a millimetre.

Then again, the purpose for which the cast was made being phrenological, the caster was evidently only concerned to obtain a replica of the actual brain-box. The face was omitted as of no importance, the cast stopping short about a centimetre below the nasion. The zygomatic arches...but one hesitates to identify as zygomatic arches the rough ledges that are left on the cast where those should be. Certainly the upper outline of what might be a zygomatic arch on the left can be traced for about three-quarters of its length, if one carefully distinguishes between it and an overlying blob of plaster on its horizontal portion; but for the rest of the distance there is merely a gap where the arch should run. On the right side, no attempt has been made to preserve anything like the shape of the zygomatic arch: the edge of the plaster ledge has been carved away roughly with a knife, and only a short "scoring" remains to mark where the middle of the upper edge of the arch met the overflowing plaster which filled up the cavity it bridges.

Turning to the base of the skull: only as far as the opisthion is there any attempt to reproduce the occipital bone. From there to about the level of the basion the skull is roughly filled in—one condyle indicated, one not—and at the basion a transverse slice is cut out right across the base of the skull. Smooth plaster fills in the space from here to the truncated nasal bones.

On the cast as it reaches us the tips of both mastoids are reduced to flat surfaces. But this is doubtless due to wear, since the cast rests upon the mastoids when it is placed upon a flat surface in the normal position, and eighty years of

shifting about on shelves will supply quite enough friction to account for the wearing down of the tips.

The caster was apparently more successful in reproducing the finer irregularities of the surface when making the occipital section than the remainder: the region of the inion, for example, shows very considerable detail, whereas the temporal and coronal sutures are almost entirely lost, and the sagittal and lambdoid likewise except in the neighbourhood of the lambda. If any of these missing features were indicated in the cast as it left the hands of its maker, they certainly have not survived the stiff coat of paint which has since been applied to the whole surface.

Such then is the very imperfect document of 1840–1. To-day we should class it contemptuously as a miserable specimen of a skull-cast. But it is only fair to the man who made it to remember that his object was different from ours, and much more limited. He is to be judged by the extent of his success in attaining the end he himself had in view. This was simply to make an accurate record of the size and shape of as much of the cranial box as was mapped out upon the current phrenological charts.

If we examine the typical chart given in the *Encyclopaedia Britannica** we see that he has covered well the phrenologically important area by taking as his limits (1) in the front, the orbital portion of the frontal bone as far as the fronto-malar sutures, and the central portion about as far as the nasion; (2) at the back of the head, a line drawn roughly across just behind the mastoids; (3) at the side, an area which includes the mastoids, goes as far as the auricular orifice, and follows a line across the temporal and sphenoid bones corresponding roughly to the line of the zygomatic arch. Two phrenological faculties he was obliged to leave quite unprovided for: the gift of languages denoted by the prominence of the eye-ball; and the capacity for recognising faces shown by the distance from the inner corner of the eye to the nose. Our worker evidently concluded that to reproduce the nose alone would not help to add this faculty to the many others which distinguished the famous doctor, for even the half-inch of stump he has left on the cast bears the mark of his knife, where he has scraped away a part of its right side†.

(*b*) *Photographs of the skull and the* 1840–1 *cast compared.*

The above description will serve to indicate the limits within which we may look for evidence concerning the skull from which this early cast was taken. Within these limits we can compare it with the recent records of the skull re-buried in July, 1922. As the existence of the 1840–1 cast was not known to us until after that date, it could not be compared with the skull direct. Our records must therefore serve instead.

To begin with, some preliminary conclusions may be drawn by the reader from a comparison of the photographs here reproduced of the skull and the 1840–1 cast.

* Under "Phrenology."

† The Royal Society of Medicine (Lond.) possesses a copy of the 1840–1 cast, presented to it by Mr Chas. Williams. It is easily seen, on comparison, that this R. S. M. cast is taken from the earlier one, and not from the skull direct: it reproduces all the casting-seams of the 1840–1, besides adding some of its own.

Each has been photographed from vertical, lateral and occipital aspects. These terms each carry with them an exact definition of the position required of the skull: it must be adjusted to the "horizontal," the "sagittal," and the "vertical" planes respectively, and these planes are determined by certain important points on the skull—the nasion, bregma, lambda, auricular points and orbital point*. Since the 1840–1 cast lacks malar bones and therefore the orbital point, gives no indication of the whereabouts of the bregma, and does not allow us to locate lambda or nasion with any exactness, we are reduced to appreciative methods in orientating it for the camera. Thus we cannot guarantee that skull and cast will be photographed from exactly similar, but from only approximately similar, points of view, and we must bear in mind, when comparing the results, that a slight shifting of the view-point means a slight—possibly more than a slight—alteration in the outline presented.

It is interesting to note here the general principle which holds in such cases: that photograph-outlines (and outlines are the chief part of what this cast will give us) can give convincing evidence of identity of shape, but, given a certain resemblance between the objects represented, cannot give convincing evidence against it! If the outlines differ, it may be due either to difference of shape in the objects, or difference of position; if they are alike, the chances are overwhelming that this is due to identity of shape.

With these considerations before us, let us now compare the corresponding Plates representing the skull, and the 1840–1 cast. We will begin with the *norma verticalis* (Plates XVII and XX). Our comparison is facilitated in this case by the fact that the length of the objects in the two plates has been made the same (173 mm.), excluding of course the nasal portion, which is incomplete in the cast. At first sight, we should say that the *proportions* of the two objects are different, and that the cast is the more dolichocephalic. We must beware, however, of the illusion produced by the different arrangements of lighting. The back and front lighting of the skull seems to shorten it, while the lighting of the sides in the cast makes this seem narrower, and it will be well to submit our impressions to the test of a millimetre rule. We find that the greatest width of the cast-photograph is 129 mm., that of the skull-photograph the same within half a millimetre (128·5 mm.). This is extraordinarily good agreement, especially when we remember that the position of the cast could only be adjusted to that of the skull by appreciative methods.

If we now compare the outlines in detail, we note how each change of direction in the one is reflected in the other; and if we test this by superposing a tracing of one upon the other, we find the two outlines are practically identical.

Other than the outline this view of the cast offers few features for comparison: the parietal foramina, quite clear in both; some markings in the region of the lambda, where, unfortunately, the photograph of the skull grows a little misty, and

* The required positions are the same as for tracing the three standard contours, and are defined in *Biometrika*, Vol. XIII. pp. 185, 187 and 194.

fails us; some idea of the moulding of the frontal region. Here too, in the frontal region, the skull-photograph is not at its clearest (the camera having been focussed more upon the detail of the sutures), but it is clear enough to enable us to recognise in the skull-forehead a good resemblance to that of the cast.

Thus, taking all the points where we can test the plates showing the vertical aspect of skull and cast, we find an agreement which it would be almost impossible to match between one skull and the cast of *another* skull, however carefully the two might have been selected.

Next the *norma occipitalis* (Plates XVI and XIX). Here the two photographs are not taken to exactly the same scale; but by measuring maximum width and median height in both cases we can readily discover whether the proportions are the same. We get:

	Maximum Width	Maximum Height	Height÷Width
Skull photograph	133 mm.	122·5 mm.	·9210
Cast „	136 mm.	124·5 mm.	·9154

We see that the ratios are very nearly the same, the difference between them amounting only to about one-half per cent. The photographer has thus succeeded in orientating skull and cast sufficiently alike to furnish those students who cannot have access to the actual objects photographed with a means of forming their own conclusions as to the relationship between them. We now turn to the outlines of the two and find close correspondence except in the region of the mastoids. The extent to which wear has abbreviated those of the cast may be judged more easily from the lateral view shown on Plate XV. The fact that the mastoids in Plate XVI (*norma occipitalis* of cast) seem to have been not only shortened but widened is due to the fact that the rough filling in between the horizon of the opisthion and that of the basion comes slightly into the picture here.

It only remains to compare the surface detail; and we shall of course ignore the seam due to bad casting which runs from mastoid to mastoid above the inion, in Plate XVI.

There is more detail available for comparison in the occipital photographs than we found in the vertical. Thus the well-defined *lineae superiores* appear quite clearly in both plates. As seen in perspective their meeting-place seems to be the inion, which is really about a centimetre higher up in the skull and projects so as nearly to cover the union of these ridges. The inion, a well-marked feature in the skull and the cast, marks the junction of the *lineae supremae*, the latter being less conspicuous than the *lineae superiores*, but quite traceable nevertheless.

Then we turn from the region of the inion to that of the lambda, and we find in the cast distinct traces of all three sutures which meet at this point. It is very unfortunate that the left lambdoid, which is clearest of those in the cast, should be confused in the other photograph by white spots which may be either fragments of plaster left on the skull after casting, or flaws in the plate used. These

have been touched up by the engraver, and give an erroneous impression of the course of this suture. The parts of the three sutures that are available for comparison, though they do not supply enough data to warrant the assertion that the sutures are identical in the two plates, certainly resemble one another, both in their general course, and in their smaller detail where we can see it clearly. In order, however, to give the student an opportunity of forming more definite conclusions as to correspondence in the lambdoid region, we have enlarged this portion of the occipital aspect of the skull as shown in a small photograph taken earlier (Plate XIII). This agrees quite well with what we see in the cast.

The parietal foramina are seen again in our *norma occipitalis* (Plates XVI and XIX), and correspond in position in the skull and cast. In the *norma verticalis* of the skull (Plate XX) the left parietal foramen was marked by a white spot which evidently was a little core of plaster left in it, for the same white spot reappeared in this foramen in Plate XIX (*norma occipitalis*). Unfortunately it was touched up by the engraver in both plates, as due to a flaw.

In this pair of photographs, as in the pair already considered, allowance must be made for different lighting effects. We have the impression that the region of the occiput protrudes more in the skull than in the cast, owing to the way in which the light throws it into relief in the former case. But we can correct this impression by referring to the lateral aspects of both (Plates XV and XVIII), where the occipital region is seen in profile.

So much for the occipital photographs. It only remains now to deal with the lateral (Plates XV and XVIII), and here we have two photographs in which the respective scales differ rather more widely. We have no difficulty in selecting one of the two measurements which will be wanted for the purpose of comparing proportions: the maximum length from glabella to occiput is an obvious choice. The other one which we should like is the height taken vertically from the auricular orifice; but this measurement is made impossible in the cast because of the lack of malar bones and our consequent inability to determine the standard horizontal plane, and from it the vertical plane also. We will accordingly take as one terminal the top of the "bump" which lies about a third of the way between bregma and lambda in each. Here we doubtless have the "vertex" of the skull, this being defined as the highest point of the skull when adjusted to the standard horizontal plane. For the other terminal, we must assume that the upper edge of the round black shadow which indicates the skull's auricular orifice corresponds in position with the upper edge of the white patch which does duty for the same feature in the cast. The results of our measurements from the vertex to the nearest point on this edge, on both plates, are as follows:

	Maximum Length	Auricular Orifice	Auricular Orifice to Vertical Maximum Length
Skull photograph	139 mm.	81 mm.	·5827
Cast „	152·5 mm.	90·5 mm.	·5934

The ratios obtained are seen to differ by one per cent, a difference quite within the limits of variation for which we should be prepared when we can only orientate for the camera by appreciative methods, and select our terminals by the same means.

We now compare the course of the two outlines: the seam in the cast across the occiput from the mastoids forms a slightly projecting ridge which hides 7–8 mm. of the natural outline immediately below the inion. The mastoid lacks its tip. In front of it the roughly modelled portion of the base shows a projecting corner, and the part of the skull carved away from this point to the nose has of course an outline that nature would not attempt to imitate. The rough work on the nose stump ascends slightly above the nasion, so that we do not get in the outline of the cast the definite kink which marks the position of the nasion in the skull. With these exceptions, all of them due to the limitations of the cast, the correspondence with the outline of the skull is very good indeed.

Finally, the surface detail must be compared. Here we must disregard the little column of plaster that starts up from the fronto-malar suture in the cast, stretches out an arm for a grip on the temporal ridge, and provides a starting-place for the seam that ends up in the neighbourhood of the auricular opening. If we can also make allowance for the knob of plaster that sits upon the external orbital angle, and the waves that flow out from what in nature is the cavity filled by the temporal muscles, we shall be able to disentangle from this riot of plaster a line which closely resembles the sweep of the one continued in the skull-photograph from the temporal ridge down along the upper border of the zygomatic arch. At about 3 cm. from the centre of the auricular orifice, however, this line breaks down altogether in the cast.

Proceeding along the same line in the other direction, namely along the temporal ridge, we can follow it for some distance both in cast and skull. We can avoid being misled by the long dark patch on the skull-frontal which seems at first sight to form part of the ridge, if we note the course of the ridge beyond this patch, for it can be followed round in Plate XVIII nearly up to the lambdoid suture.

On the occipital bone we can see in both lateral photographs traces of the left *linea superior*, corresponding as far as they go. Of the coronal and lambdoid sutures nothing is seen in the cast-photograph (Plate XV); of the temporo-parietal only a little; and that little, we admit, does not look much like what we see in the skull (Plate XVIII). Nor does the posterior outline of the left mastoid where it merges into the temporo-occipital suture look the same in both plates.

When we sum up the evidence, however, yielded by our detailed comparison of the three pairs of photographs of skull and 1840–1 cast, we find that the two discrepancies noted are vastly outbalanced by the mass of points in which the cast, within its own limitations, agrees with the skull. And we conclude that these discrepancies must be due to imperfect workmanship.

(*c*) *Comparison between* 1840–1 *and* 1922 *casts.*

We have chosen to use the photographs, rather than our other records, for the detailed examination of points which these and the others supply, because the photographs are available for all who wish to study the subject. There are some tests, however, which can only be applied direct to the 1840–1 cast and to our own plastic record of the skull, the cast of 1922. Photographs can testify to correspondence in shape, but not in size; and even as regards proportion, these two sets do not show more than a close approximation to one another owing to the factor of non-identical position. Again in comparing the two solid objects one finds similarities in contour and detail which do not appear in the photographs: thus, for instance, there is a small but distinct depression in the coronal suture of the 1922 cast just above where it crosses the right temporal ridge, and we find this depression in a similar position on the 1840–1 cast; then, on the right side, the course of the anterior portion of the temporal suture is shown in the earlier cast, and it corresponds with that in the recent one. But to enumerate all the further similarities between the two casts, even where these lend themselves to verbal description—and many do not—could serve no useful purpose. And we will say briefly that the evidence given by similarities in the photographs finds ample further confirmation in the casts. And, as regards size, we have taken on both casts all the standard measurements that the 1840–1 cast would allow and half a dozen others, and we find them to agree.

There is no other conclusion possible than that we have in the skull sent to us the original of the cast presented to the Norfolk and Norwich Museum in Oct. 1841. In other words, if the cast said to be taken of Sir Thomas Browne's skull in 1841 is that of Sir Thomas Browne's skull, then the skull recently returned to earth was certainly that of Sir Thomas Browne.

Measurements of skull and cast compared.

Before leaving the subject of the casts we will put on record a few of the major measurements taken on the 1922 cast to compare with those taken on the skull direct.

		Skull	1922 cast
Cranium	Maximum length (*L*)	194·6 mm.	199·0 mm.
	„ breadth (*B*)	143·7 „	148·8 „
	Zygomatic breadth (*J*)	132·3 „	136·0 „
	Basio-bregmatic height (*H*)	121·9 „	120·9 „
	Nasal height (left) (*NH*, *L*)	52·2 „	52·5 „
	Basion to nasion (*LB*)	102·7 „	101·2 „
	Length of arc, nasion to opisthion (*S*) ...	380 „	383 „
Mandible	Outside condylar width (w_1)	120·8 „	121·6 „
	Projective length of mandible (*ml*) ...	105·2 „	107·1 „
	Gonial angle (*M*∠)	118·5°	116·0°

This will give some measure of the spread of the plaster after the cast has been made, and show the unreliability of measurements taken from such a cast. In this case the length of the skull has been increased by 4·4 mm. The spread is longitudinal and lateral: the height has not been increased, but, on the contrary, slightly lowered. Perhaps the direction of the spread was due to the position in which the cast would rest.

Whether our figures give a measure by which to estimate the order of the differences to be expected generally between skull and cast we cannot say. It is interesting to note, however, that the measurements taken on the 1840–1 and 1922 casts agreed: in these two cases, at any rate, the spread was the same.

(V) The Portraits and the Skull compared.

Our study of the portraits eliminated all but four from final consideration: many were demonstrably copies, and some were alien portraits mis-named Sir Thomas Browne. Of the four that remain, the Wellcome portrait makes a claim to represent him that is extremely doubtful, and it only stays to get, if possible, some support from the skull. As regards the other three—the Norwich painting, the Buccleuch miniature, and the pencil portrait at the National Portrait Gallery—these are all undoubtedly portraits of Sir Thomas Browne; it is very improbable that they were all done from life, or were all independent of one another, but the nature of the relationship between them has not been definitely established. In the case of these three portraits, we hope that a comparison with the skull will help to some extent to decide which represents most faithfully the face of the living man.

In order to facilitate our comparison, the cast of the skull has been photographed "three-quarter face" (Plate XXVIII). The position has been made the same as in the Norwich and miniature portraits as nearly as this could be estimated, but it cannot be guaranteed identical, and there is the possibility of enough difference to be misleading. If, for example, we had tilted the skull somewhat further forward we should have seen more of the top of the head, and thus increased slightly the apparent height of the forehead. Nothing could make it seem high, but the lowness might perhaps have been made rather less conspicuous.

Before studying the portraits, it will be well first to decide from the skull what characters we shall expect to find in the living head. To begin with that part of the vault of the skull which lies between the temporal ridges and descends to just above the supraciliary ridges, we shall expect the conformation of the living head to follow closely that of the underlying bone here, the thickness of the scalp being slight, and fairly even throughout this area.

The living forehead, then, must have appeared low. Must it also have appeared narrow? for we shall see that Sir Thomas Browne's minimum forehead breadth (*B′*), measured between the temporal ridges, was considerably less than the normal. Now we notice in the skull that the temporal ridges, having approached to within their minimum distance apart, diverge rather more rapidly than is usual below that

point. Thus the width of the frontal at the level of the upper margins of the orbits is more normal than it is higher up. We shall therefore not expect the face to look unusually narrow at the level of the eyebrows. Again we notice that the general direction of the outline of the skull as seen in the horizontal contour (Plate XXX), after it has turned the corner at the temporal ridges, is not directed so sharply to the rear as usual; and that just beneath the temples there is a "bulge" in the bone that must push outwards that part of the temporal muscle which covers it. Thus, though the position of the temporal ridge will mark a change of direction in the horizontal contour of the living head, that change will not be abrupt, and, in addition, the width at the temples will help to take away the impression of narrowness in the forehead.

Still considering the question of width, we see that the malar bone, as seen in this "three-quarter" photograph of the skull, projects further than the upper part of the orbit. The amount of projection was increased, not diminished, by turning the skull either slightly more towards the observer, or more away from him. Now there is not normally much thickness of flesh over either bone at the spot silhouetted in our photograph. We shall expect therefore that in the living face, as seen from this aspect, the cheek-bone would project somewhat.

Turning to the nose, we find the nasal bones thrust forward at a considerable angle from the plane of the face. This of course will not represent the line of the nose, the thickness of flesh over the nasion being considerably more than lower down: over the tip of the nasal-bones there is in fact little but skin. It is evident, however, from the position of this tip, that the nose must have been either prominent or aquiline, the choice depending upon the shape of the cartilage which supported the lower portion.

Let us now see how far these expectations are fulfilled by the various portraits, beginning with the Wellcome (Plate IX). It is quite certain that Sir Thomas Browne's forehead *cannot* have looked like the one shown in this portrait. If we had other evidence to prove that this was intended for his portrait, the picture itself would not be disproof, for many misrepresentations, intended and otherwise, can find their way into a portrait. But if this picture looks to the evidences of its own features to support a very doubtful claim, it looks in vain.

Comparing now the other three portraits with our deductions from the skull, and considering first the shape of the forehead, the miniature seems to me to give more of an impression of lowness than the other two. By bending the head slightly forward the artist gets in the pencil portrait a legitimate addition to the apparent height, but I think he has also added somewhat more than this. None of the artists has been *quite* candid about the depressed forehead of the subject; but the miniature seems to have disguised the depression least.

With regard to narrowness of forehead, we have seen that this would probably not be so conspicuous as the B' measurement alone would lead us to expect. We may notice, however, that the miniature and the Norwich painting have a shadow on the forehead to our left. This was not caused by the hair, for the light is not falling

from that direction. It indicates therefore a change in the direction of the contour of the brow; not an abrupt change, and one which does not take place as far down as the eyebrows. The position and appearance of this shadow fit in quite well with what we should expect from our examination of the skull. The plumbago drawing (Plate I, Fig. 1) gives no such indication and is, I think, less like the original in this point. About the malar region none of the portraits indicates any projection as compared with the part just above the eye. In the miniature, in fact, the cheek-bone seems to retreat slightly in comparison. It looks as though the miniature were farther from the fact, in this particular, than either of the other two.

As regards the nose, only one portrait satisfies us: the Norwich. Here the nose is aquiline. In the others it is neither aquiline nor prominent, and we are sure it must have been one of these, in life.

Summing up then, we surmise that the Norwich portrait is most like its subject in the shape of the nose; that it shares with the miniature the recommendation of being truthful in the temporal region of the brow, which the pencil portrait does not; that it and the plumbago drawing seem rather the more faithful—though not conspicuously so—in the region of the cheek-bones; but that the miniature is the one which gives us the truest indication of the slope of the forehead.

(VI) The Ancestry of Sir Thomas Browne.

The name *English* covers a variety of racial stocks, which still, in spite of some blending, differentiate the mental and physical characters prevailing in the different corners of England. To what stock or stocks does Sir Thomas Browne belong? The accompanying pedigree makes some contribution to our information on this point. The pedigree has been compiled from various sources and claims no authority of its own. It is left for some later student to amplify, and possibly amend, by means of direct research among registers, wills and other records such as the present worker has been unable, through lack of time, to explore.

A glance at the family-tree before us shows that most of the ancestors whose names are here set forth belonged to the class called the "squirearchy." We must beware however of concluding that these are representative of the large majority who go unrecorded. That few of the latter belonged to the ranks of the nobility we may reasonably assume, for such would surely figure in the pedigrees handed down. But it is quite probable that many of the blanks in the table before us are due to the circumstance that the families concerned were of lower status than the squirearchy, and less likely to preserve their family records.

On his father's side Sir Thomas Browne was descended mainly from Cheshire folk. The records of the male line go back only a few generations, and the reason is clearly that given above. We learn from Ormerod (Vol. II. p. 819) that until after the dissolution of the monasteries the Brownes were only fee-farmers at Upton, holding their lands in fee to the Abbey of St Werburgh at Chester. After the dissolution came "struggles between the Dean and Chapter, and Sir Richard

Cotton"; and in the 22nd year of Elizabeth (1580–1) the abbey lands "were confirmed to various fee-farmers, the most considerable of whom were the Brownes and the Brocks."

Of the families from whom Sir Thomas' paternal grandmother could trace her descent many records exist in Ormerod's *History of the County of Cheshire*. Evidently they were "county families" in the main, the predominant county being Cheshire, though we find one of her four grandparents coming from Essex*. There is also a great-great-grandmother of hers who takes the pedigree into Lancashire, and earlier still we note infusion from Flintshire, and before that, from Staffordshire. We may safely say, however, that the majority of her ancestors, for some generations back (as doubtless also those of her husband), were Cheshire-born and Cheshire-bred. Eighteen generations from Sir Thomas Browne—sixteen from his grandmother—take us back to the Conquest, and here we find, in several instances, that Norman knights were founders of the families whose blood was united in the veins of Sir Thomas Browne.

So much for his father's folk. As regards his mother, biographers of Sir Thomas Browne generally assign the Garraway family to Lewes in Sussex, their authority being found in a reminiscence occurring in one of his letters: "I remember, when I was very yong, and I thinck butt in coates, my mother carryed mee to my grandfather Garawayes howse in Lewys†"; also in the fact that the brief pedigree drawn up by Sir Thomas in 1664 gives his mother's name as "Anne, daur. of Paul Garraway, of.....in Coun. Sussex." (The blank is his.) But we should be very much mistaken if we assumed the Garraway connection with Sussex to be as intimate as that of the Brownes with Cheshire. We see illustrated, in fact, in this family, the mixing process which was continually at work in the metropolis, and extended in some degree to the counties round about. Wherever Anne Garraway herself may have been born and lived until her marriage, certainly the designation "of Sussex" would require considerable qualification if applied to her parents‡. The birthplace of Anne's mother has not been traced: we only know that she had been married previously, and that her first husband is described as "of London and Iver, Co. Bucks." Anne's father, Paul Garraway, was born at Acton, Middlesex (1553), married her mother at St Peter-le-Poor, London (1582), married a second time at Tingrith, Bedfordshire (1611)§, and was buried at St Martin's-in-the-Fields, London (1620), being described in the register there as "late of Tingridge, Co. Beds." That he lived for a time in Sussex we know only from Sir Thomas Browne's recollection of the visit to the "howse in Lewys." At the time of this visit the child must have been, as he said, "very yong," and quite probably still "butt in coates,"

* Ormerod says a Horncliffe of Hallowbury in Essex. There are a Great and a Little Hallowbury on the borders of Essex near Bishop's Stortford. I have found no record of the Horncliffes among the Essex pedigrees examined.

† Letter to his son Edward, Jan. 9, 1681–2. MS. Sloan 1847.

‡ The information here given with regard to the ancestors of Anne Garraway is derived from the unpublished pedigree compiled by R. Garraway Rice, Esq., F.S.A. Part of this has been taken from Harleian MS. No. 1551, p. 65 (Brit. Mus.).

§ *Genealogia Bedfordiensis*, by F. A. Blaydes, 1890, p. 294.

for he was not yet six years old when his grandfather married again and went to live at Tingrith. Whether the latter's stay in Lewes had been long we do not know. At any rate it is obvious that his* connection with the county was not such as to warrant our counting Anne Garraway as of Sussex stock: and we must look further back in her pedigree.

In the male line this carries us to Watkin Garraway of Weobley in Herefordshire (d. 1519). This family owned lands in Weobley which later in the sixteenth century were acquired by their neighbours, the Brydges, who had possessed the adjacent land called The Ley at least since 1428†. Here we come to land-owning families such as we met with in Cheshire, and we are probably safe in assuming that they had lived and died in Herefordshire for many generations, and that their wives were taken from this or neighbouring counties.

A younger son of Watkin Garraway, who migrated to London, took to wife one of a branch of the Brydges family which had also settled in the capital: the daughter of Sir John Bruges (or Brydges), Lord Mayor of London in 1520. Her mother was an Essex woman, whose people, said to belong originally to Kent, had been settled in Essex for several generations at least.

Thus we may count their son, who was the father of Paul Garraway, as three parts of Herefordshire stock, one part of Essex. He married a Middlesex woman, who brought into the family property at Church Acton, Middlesex, and in this parish their son Paul (or Powle) was baptized.

It follows that Sir Thomas Browne's ancestry was drawn from many districts in England. If we estimate the proportions which various counties contribute, according to what we know of his 16 great-great-grandparents, we get as result: 7 parts Cheshire (with infusion from Lancashire and Flintshire), 1½ parts Herefordshire, 1½ parts Essex, 1 part Middlesex, and 5 parts unknown but probably of the home counties.

It would have been interesting, from another point of view, to discover among the members of this family other distinguished names; but it must be confessed that our search reveals none. We note that the Huxleys of Cheshire are among the ancestors of Sir Thomas Browne in two lines, and it is most probable that the great scientist who has made the name of Huxley famous is another descendant of that same family. Even if the fact were definitely established‡, however, the link between these two men could hardly fail to be very remote.

The pedigree we have given includes only the names of Sir Thomas' ancestors. For the complementary table of his descendants we refer the student to the

* Some of his relations were more closely connected with Sussex than he. His uncle, Bartholomew Garraway, had married a Sussex woman, and died possessed of considerable properties in the county (including a mansion-house at Southover, Lewes), part of which came to him as her dowry. (Will of Barth. Garraway, 10 Nov. 1607, proved in P.C.C. 19 Feb. 1607/8 [9 Windebancke]). Another uncle, Anthonie Garraway, was vicar of Iford, Sussex, and died 1614.

† *Mansions and Manors of Herefordshire*, by Ch. J. Robinson, p. 290.

‡ [An attempt to link up, through the Macclesfield registers, T. H. Huxley's grandfather Thomas Huxley of Coventry with Samuel Huxley, Mayor of Macclesfield in 1746 (who was connected with the Cheshire Huxleys), was unsuccessful. Ed.]

"Pedigree of Sir Thomas Browne" brought together by the researches of Mr Charles Williams*. But one descendant, his son Dr Edward Browne, figures in our own pages (Plate XII), to demonstrate the fact that Sir Thomas passed on to this member of his family, not only a portion of his own ability, but also, in conjunction with it, his own depressed and sloping brow.

(VII) STUDY OF THE SKULL.

The skull of Sir Thomas Browne is distinguished by certain features sufficiently unlike those which we are accustomed to find among the majority of skulls, to attract our attention at the first glance. These features were observed and described by Robert Fitch in 1840. But though his description serves quite well as a contribution to the evidence by which the identity of the skull was established, both his and our impressions of unusualness need to be submitted to the test of exact measurement. We have, fortunately, two series of seventeenth century London crania available for comparison: I refer to the Whitechapel and Moorfields series† on which a large number of measurements were taken by the late W. R. Macdonell. As the Whitechapel is the considerably longer of these two series, we may consider the data it supplies the most reliable craniological evidence we possess concerning the London contemporaries of Sir Thomas Browne.

Outside London we have very little in the way of data to which we can turn. Two long series of skulls have been dealt with by Professor F. G. Parsons—the Hythe (Kent) and Rothwell (Northants) crania‡. Only six measurements were taken on each however; and of these, the auricular height is not measured according to the convention used for Sir Thomas Browne and the two London series, and would, therefore, have to be omitted.

Again, we are given only the *means* of the six measurements taken; the *standard deviations* are not supplied, nor are we able at the present to remedy this omission without very considerable computations. We are thus left without the means of gauging the probable significance of any differences that exist between Rothwell and Hythe values and any others. Even had we all the data we could wish, however, concerning these Northamptonshire and Kentish groups, they would be not more but rather less appropriate than a London series, as our basis of comparison. We have seen that Sir Thomas Browne's ancestry was not drawn from one county, but from half-a-dozen at least. In him was blended, to our knowledge, the N.W., W., and S.E. of England; and if our information as to his ancestry were more complete, we might have to add other parts of England to the list. Now although the London of the seventeenth century was not the melting-pot of local strains that it is to-day, it was doubtless more so than any other one district we could choose. In using a London series for our standard of comparison, therefore, we are using what would be most aptly chosen if we had any choice.

* *Norfolk Archaeology*, Vol. xv. 1904, pp. 109—113.

† *Biometrika*, Vol. III. p. 191 and Vol. v. p. 86.

‡ *Journ. Anthrop. Inst.* Vol. XXXVIII. (1908), p. 419 and Vol. XL. (1910), p. 483.

With considerable satisfaction at our good fortune in having data to hand so suitable for our purpose, we now turn to the examination of these, first putting on record in Table I the full series of measurements* obtained from the skull of Sir Thomas Browne.

The Whitechapel group contains 138 male crania, or parts of crania. In dealing with this group, Macdonell took 26 measurements direct from the skull, and calculated from them 16 others (5 angles and 11 indices), making 42 measurements in all. Two of these will not be used in our comparative Table II A, since the alveolar portion of Sir Thomas Browne's upper jaw was too far absorbed to allow any measurement of palate breadth (G_2); and the index 100 H/B is not necessary when one has already 100 B/H.

In making his *Second Study of the English Skull*, with the Moorfields crania for his material, Macdonell measured 8 additional characters, obtaining their values for 34–44 individuals. For these 8 characters we shall use Moorfields means and standard deviations as our basis of comparison (Table II B), in default of constants obtained from the larger London group.

Thus we have in all 48 means and standard deviations based on numbers ranging from 34 to 138, to do duty for the corresponding constants of the total population they represent. Now, in any of these 48 characters, given the difference between Sir Thomas Browne's measure and the mean measure for the total population with which we wish to compare it, the probability that this difference is merely due to the chances of a normal distribution could be readily found† by expressing the difference in terms of the corresponding standard deviation. Obviously these constants for the total population are unknown. But our substitution of the means and standard deviations of the samples measured will only affect the resultant figures to an unimportant extent; the probable error of the mean of samples containing over 45 individuals (as do nearly all of ours) is less than ·1 times the S.D., and the probable error of the S.D. for the same number is less than ·07 times the S.D.‡ In 9 characters only out of our total of 48 does the number of individuals measured fall below 45; and in the smallest number used (34) the probable error of the mean of the sample is ·11567 times the S.D., and of the S.D., ·08179 times. Variations of this order will not make the difference between significance and insignificance in the figures obtained for our $\frac{B-\overline{W}}{\sigma_W}$ and $\frac{B-\overline{M}}{\sigma_M}$ in Tables II A and II B.

In our last column of these tables we have entered the proportion of London population of that time which may be expected to possess characters differing from the mean in the same way and to the same extent as, or to a greater extent than, Sir Thomas Browne. This has been done only where the proportion was less

* For full definitions of measurements used see *Biometrika*, Vol. XIV. pp. 196—198, 257—259.

† See *Tables for Biometricians and Statisticians*, ed. by Karl Pearson, 1914, p. xvii and Table II.

‡ *Loc. cit.* p. xxii and Table V.

TABLE I. *Measurements of the Skull of Sir Thomas Browne.*

I. Cranium.

		mm.
(*F*)	Ophryo-occipital length ...	190·0
(*L'*)	Glabellar projective horizontal length...	195·1
(*L*)	Maximum length from glabella to occiput	194·6
(*B*)	Maximum horizontal breadth on parietal bones	143·7
(*B'*)	Least forehead breadth ...	87·4
(*H'*)	Basio-bregmatic height ...	121·9
(*H*)	Height from basion to point vertically above it (with skull adjusted to Frankfort horizontal plane)	122·0
(*OH*)	Auricular height	102·6
(*LB*)	Basion to nasion	102·7
(*Q*)	Transverse arc through apex terminating at top of craniophor ear-rods	293·2
(*Q'*)	Transverse arc terminating at auricular points	297
(*S*)	Sagittal arc from nasion to opisthion	380
(S_1)	Nasion to bregma	129
(S_2)	Bregma to lambda	123
(S_3)	Lambda to opisthion	128
(S_3')	Chord from lambda to opisthion	99·8
(*U*)	Horizontal circumference above supraciliary ridges and round the most projecting part of occiput	549
(*PH*)	Alveolar point to tip of anterior nasal spine	21·7 (?)
(*G'H*)	Nasion to alveolar point ...	73·1 (?)
(*GB*)	From lower end of one zygomatic-maxillary suture to that of the other, where sutures cross lower front rim of cheek-bone	90·9
(*J*)	Zygomatic breadth	132·3
(*NH, R*)	Nasion to lowest edge of pyriform aperture, right ...	52·2
(*NH, L*)	Nasion to lowest edge of pyriform aperture, left	52·2
(*NB*)	Greatest breadth of pyriform aperture	22·6
(*DS*)	Dacryal subtense	12·8
(*DC*)	Dacryal chord	20·9
(*DA*)	Dacryal arc	32·7
(*SS*)	Simotic subtense	5·0
(*SC*)	Simotic chord, i.e. minimum between the two naso-maxillary sutures	8·7
(O_1, R)	Greatest breadth of right orbit (using Fawcett's curvature method)	43·7
(O_1, L)	Greatest breadth of left orbit...	42·8
(O_2, R)	Greatest height of right orbit taken perpendicular to O_1, R	36·3
(O_2, L)	Greatest height of left orbit taken perpendicular to O_1, L	36·7
(G_1)	Length of palate from point of *spina nasalis posterior* to an imaginary line tangential to the inner rims of the alveoli of middle incisors	53·3 (??)
(G_1')	Similar to G_1 but taken from base of spine	49·0 (??)
(G_2)	Breadth of palate between inner alveolar walls at 2nd molars.	37·5 (??)
(*GL*)	Basion to alveolar point ...	98·3 (?)
(*fml*)	Length of *foramen magnum* from basion to opisthion	34·6
(*fmb*)	Greatest breadth of *foramen magnum*	30·1
($P\angle$)	Angle between line joining nasion to alveolar point, and horizontal	84·7° (?)
($N\angle$)	Angle between lines joining nasion to basion and alveolar point respectively	65·4°
($A\angle$)	Angle between lines joining alveolar point to basion and nasion respectively	71·9° (?)
($B\angle$)	Angle between lines joining basion to alveolar point and nasion respectively	42·7° (?)
(θ_1)	Angle which line joining nasion to basion makes with horizontal, i.e. $(180° - P\angle - N\angle)$ ...	29·9° (?)
(θ_2)	Angle which line joining basion to alveolar point makes with horizontal, i.e. $(P\angle - A\angle)$ ...	12·8° (?)
	Weight of cranium	622·5 grs.
(*C*)	Capacity of skull	1509 cms.^{3}

Indices.

$100\frac{B}{L'}$	73·7
$100\frac{H}{L}$	62·7
$100\frac{H}{L'}$	62·6
$100\frac{B}{H}$	117·8
$100\frac{G'H}{GB}$	80·4
$100\frac{NB}{NH}$	43·3
$100\frac{O_2}{O_1}R$.....	83·1
$100\frac{O_2}{O_1}L$	85·7
$100\frac{fmb}{fml}$	87·0
$100\frac{DS}{DC}$	61·2
$100\frac{SS}{SC}$	57·5
Oc. I. i.e. $100\frac{S_3}{S_3'}\sqrt{\frac{S_3}{24\,(S_3 - S_3')}}$	55·8

TABLE I (*continued*).

II. Mandible. (*Biometrika*, Vol. XIV. pp. 253—258.)

(The alveolar rim of the mandible was so far absorbed that several measurements could not be taken.)

		mm.
(w_1)	Greatest width from outside one condyle to outside of the other	120·8
(w_2)	Greatest width from outside one angle to outside of the other ...	100·2
(zz)	Least distance between inner rims of *foramina mentalia*	45·2
$(c_r c_r)$	Coronion to coronion	101·6
(rb)	Least breadth of left ramus parallel to standard basial plane ...	35·1
(rb')	Least breadth of ramus in any direction	32·0
$(c_y c_r)$	Condylion to coronion	32·6
$(g_o g_o)$	Gonion to gonion	97·0
$(g_n g_o (l))$	Gnathion to left gonion	89·8
$(g_n g_o (r))$	Gnathion to right gonion ...	89·0
$(c_y l)$	Greatest length of left condyle ...	18·3
$(c_y b)$	Greatest breadth of left condyle perpendicular to $c_y l$	6·6
$(p_a g_n)$	Pogonion to gnathion	8·4
$(g_o p_a g_o)$	Arc measured with steel tape from one gonion to the other through the pogonion	202·5
(ih)	Least height of incisura (left) above standard basial plane ...	50·2
(ih')	Greatest depth of incisura (left) from line joining condylion to coronion	9·9
$(c_r h)$	Height of coronion (left) above standard basial plane	62·4
$(c_y h)$	Height of condylion (left) above standard basial plane	58·0
$(c_y l)$	Length of corpus	80·1
(rl)	Length of ramus (left)	61·8
(ml)	Projective length from line joining posterior points of condyles to pogonion	105·2
$(M\angle)$	Angle between left ramus tangent and standard basial plane ...	118·5°
$(R\angle)$	Angle between condylo-coronoidal line and left ramus tangent ...	69·6°
$(G\angle)$	Angle between the lines joining gnathion to right and left gonia respectively	65·5°
	Weight of mandible	63 grs.

Indices.

$100\,\frac{c_r h}{ml}$	59·3	$100\,\frac{c_y b}{c_y l}$	36·1
$100\,\frac{c_r c_r}{ml}$	96·6	$100\,\frac{g_o g_o}{c_r c_r}$	95·4
$100\,\frac{g_o g_o}{c_y l}$	121·1	$100\,\frac{c_y h}{c_r h}$	92·9
$100\,\frac{rb'}{rl}$	51·8	$100\,\frac{ih'}{c_r c_y}$	30·4

than 1 in 25—a figure that has been chosen to bring it roughly into line with the standards used when comparing two sample populations. It is reckoned, as a rule, that when the chances are only 1 in 22 (i.e. when the difference between the two means equals 2·5 times its probable error) in favour of their being part of the same population, these figures are possibly significant of their belonging to *different* populations. When the chances are reduced to 1 in 46 (3 times prob. error), the disproportion is counted quite probably significant.

We will examine first of all the way in which the *absolute* measurements of Sir Thomas Browne's skull compare with the mean values for contemporary Londoners as shown in the above tables. We see that in the great majority of characters he is entirely normal and usual: only four measurements demand any enquiry into their possible significance, viz. B' (least forehead breadth), H' (height of skull from basion to bregma), OH (vertical height above the ear-rods of the craniophor when the skull is orientated to the Frankfort plane), and G_1 (length of palate from tip of posterior palate spine).

TABLE II A.

Comparison of Sir Thomas Browne's Skull with Whitechapel Series (Males).

Character	Sir Thomas Browne (B)	Whitechapel Series			Deviation of Browne from Mean ($B - \overline{W}$)	Deviation in terms of s.d. ($[B - \overline{W}]/\sigma_W$)	Expected frequency where less than 1 in 25
		No.	Means ($\overline{W}$)	Standard Deviations (σ_W)			
C	1509	72	1476·9	122·37	+32·1	+0·3	
F	190·0	138	187·4	6·17	+ 2·6	+0·4	
L'	195·1	72	187·8	5·64	+ 7·3	+1·3	
L	194·6	137	189·1	6·27	+ 5·5	+0·9	
B	143·7	135	140·7	5·28	+ 3·0	+0·6	
B'	87·4	132	98·0	4·20	−10·6	−2·52	1 in 170
H'*	121·9	122	132·0	5·56	−10·1	−1·82	1 in 29
OH	102·6	135	112·1†	4·28	− 9·5	−2·22	1 in 76
LB	102·7	119	101·6	4·13	+ 1·1	+0·3	
Q'‡	297	115	307·9	11·40	−10·9	−1·0	
S	380	131	377·1	13·69	+ 2·9	+0·2	
U	549	131	524·3	15·02	+24·7	+1·6	
$G'H$	73·1 (?)	75	70·2	3·86	+ 2·9 (?)	+0·8 (?)	
GB	90·9	55	90·9	5·07	0	0	
J	132·3	43	130·1	5·57	+ 2·2	+0·4	
NH	52·2	79	51·2	2·60	+ 1·0	+0·4	
NB	22·6	70	24·3	2·16	− 1·7	−0·8	
O_1, R	43·7	68	43·0	2·02	+ 0·7	+0·3	
O_1, L	42·8	63	43·1	1·81	− 0·3	−0·2	
O_2, R	36·3	69	33·4	2·22	+ 2·9	+1·3	
O_2, L	36·7	67	33·5	1·88	+ 3·2	+1·70	
G_1	53·3 (??)	69	48·3	2·74	+ 5·0 (?)	+1·82 (?)	1 in 29 (?)
G_1'	49·0 (??)	69	44·7	2·59	+ 4·3 (?)	+1·66 (?)	
GL	98·3	73	95·9	4·49	+ 2·4	+0·5	
$P\angle$	84·7° (?)	63	86·1°	3·92	− 1·4° (?)	−0·4 (?)	
$N\angle$	65·4° (?)	69	65·2°	3·52	+ 0·2° (?)	+0·1 (?)	
$A\angle$	71·9° (?)	69	73·4°	3·41	− 1·5° (?)	−0·4 (?)	
$B\angle$	42·7° (?)	69	41·4°	2·50	+ 1·3° (?)	+0·5 (?)	
θ_1	29·9° (?)	59	28·7°	2·53	+ 1·2° (?)	+0·2°	
θ_2	12·8° (?)	59	12·9°	3·34	− 0·1° (?)	0·0 (?)	
100 B/L'	73·7	69	75·2	2·97	− 1·5	−0·5	
100 B/L	73·8	131	74·3	3·26	− 0·5	−0·2	
100 H'/L'	62·5	69	70·4	2·67	− 7·9	−2·96	1 in 649
100 H'/L	62·6	120	70·0	3·22	− 7·4	−2·30	1 in 93
100 B/H'	117·8	116	106·3	5·11	+11·5	+2·25	1 in 82
100 $G'H/GB$	80·4	53	76·5	5·39	+ 3·9	+0·7	
100 NB/NH	43·3	70	47·6	4·58	− 4·3	−0·9	
100 O_2/O_1, R	83·1	68	77·7	4·66	+ 5·4	+1·2	
100 O_2/O_1, L	85·7	63	77·9	3·78	+ 7·8	+2·06	1 in 50

* Macdonell's H is the same measurement as our H'.

† *Re* Macdonell's OH as here corrected, see *Biometrika*, Vol. XIII. p. 217.

‡ Macdonell used the bregma in measuring the transverse arc (his Q), instead of taking this measurement in the vertical plane, as is done now. It so happens that in Sir Thomas Browne's skull the bregma is very nearly vertically over the auricular axis (see Plate XXIX), so that his Q gives the same results in this skull as our Q'.

TABLE II B.

Comparison of Sir Thomas Browne's Skull with Moorfields Series (Males) as regards Characters not measured on Whitechapel Skulls.

Character	Sir Thomas Browne (B)	Moorfields Series			Deviation of Browne from Mean ($B - \overline{M}$)	Deviation in terms of S.D. ($[B - \overline{M}]/\sigma_M$)	Expected frequency where less than 1 in 25
		No.	Means ($\overline{M}$)	Standard Deviation (σ_M)			
S_1	129	44	129·3	5·55	−0·3	−0·1	
S_2	123	43	128·7	7·61	−5·7	−0·7	
S_3	128	40	120·5	8·48	+7·5	+0·9	
S_3'	99·8	40	98·1	5·94	+1·7	+0·3	
fml	34·6	36	35·4	2·86	−0·8	−0·3	
fmb	30·1	34	29·7	1·82	+0·4	+0·2	
100 *fmb*/*fml*	87·0	34	84·2	6·64	+2·8	+0·4	
Oc. I. (*CC*)	55·8	40	58·5	2·15	−2·7	−1·3	

The last of these can be dismissed at once from serious consideration. Its value as given for Sir Thomas Browne is marked with a double query, owing to the fact that the exact position of both terminals could only be guessed at: the tip of the palate spine had vanished, and the loss of the upper incisors had been followed by the disappearance of their alveoli through absorption. Were the measurement here given absolutely reliable, the deviation of Sir Thomas Browne from the mean in this character only borders on what might be judged significant, for 1 in 29 of the population may be expected to possess as long a palate or longer; while a reduction of 1 mm. in the length (and we cannot guarantee the accuracy of this ?? measurement within a millimetre) would make it quite normal.

There remain the three measurements B', H', and OH. In the first of these the deviation is certainly striking: the minimum width of Sir Thomas Browne's forehead is 10·6 mm. less than the average, and only 1 in 170 of the population may be expected to fall below the mean to this extent. Suspending comment on this discovery until the other singularities of the skull have been examined, we turn to the characters OH (vertical auricular height) and H' (basio-bregmatic height), and find that each falls short of its mean value, OH by 9·5 mm., H' by 10·1 mm. The auricular height and the basio-bregmatic height of a skull will obviously be influenced to a considerable extent by the same factors, and when we find one of them differing widely from the mean, we expect to find the other differing, as here, in the same sense. It would be interesting to know whether the whole of the deficiency in our H' can be attributed to the portion of it which lies above the level of the auricular point; but we must wait for this until the complete sagittal type contour for the seventeenth century English is some day worked out*. I have to thank Mr Geoffrey Morant† for working out the position of the auricular

* See p. 64.

† I am also indebted to Mr Morant for his kindness in revising my measurements. I have to thank Mr E. S. Pearson for his photographs of the skull and casts and Miss Ida McLearn for her drawings of the skull contours and the preparation of the pedigree.

point, as a step towards the completion of this contour. From this we see that the bregma of the English contour lies 16 mm. in front of the vertical through the auricular point. Had bregma, auricular point, and basion lain practically in the same vertical plane, as in Sir Thomas Browne's skull, we should have been able from our figures to apportion correctly Sir Thomas' deficit in height of skull, and say whether any at all was sub-auricular.

However, from the fact that the deficiency in OH is such as would be expected in only 1 in 76 of the population represented by the Whitechapel skulls, while an H' no longer than this one would be found in 1 in 29, it seems probable that the sub-auricular portion of H' is the more normal.

Our comparison of the absolute measurements given in Tables II A and II B thus yields two differences only which are markedly unusual: the forehead is narrower, and the height of the skull less, particularly above the ear-passages. It is possible, however, that unimportant differences in absolute size may cause noteworthy differences in the proportions of skulls. For comparison from this point of view we turn to the indices given in our tables. Here we find four in which our figures indicate significant differences. Into three of these the factor height (H') enters: 100 H'/L', 100 H'/L, and 100 B/H', and from them we see that the small height is more uncommon in conjunction with the comparative broadness and length of the skull than it is as an absolute value. Skulls which are platycephalic to this degree (indicated by 100 B/H') or more, might be expected in only 1 in 82 of the population. As regards the height in proportion to length, we have two indices giving a measure of the chamaecephaly of the skull, according to the two different methods by which the length of skull is measured. The two indices are practically the same for Sir Thomas Browne; and they are also practically the same for the Whitechapel mean. But the values of L' having been found less variable than those of L in the Whitechapel series we get standard deviations for 100 H'/L' and 100 H'/L, of 2·67 and 3·22 respectively. Measured by these standards, it would seem that the chances are 1 in 649 in favour of the occurrence of skulls with Sir Thomas Browne's 100 H'/L' index or less, and 1 in 93 in favour of his 100 H'/L or less, in the total population. It must be remembered however that these figures are only exact if we assume that the sample measured is exactly representative of the total population from which it is drawn. All that we know is that the chances are strongly in favour of its being very nearly representative, when the number of cases measured is 69 (as in L'), but still more strongly when the number is increased to 120 (as in L).

We are therefore bound to take 1 in 93 as the truer measure of the chances of the occurrence of chamaecephaly as great as or greater than Sir Thomas Browne's; and to suppose that the 69 skulls which were sufficiently complete to be adjusted on the craniophor for the measurement of L' give us the mean value correctly, but show somewhat little variation from it. In fact that they are probably not only a very average lot, but rather more average than is usual!

One other index remains: that of the left orbit (100 O_2/O_1, L). Sir Thomas Browne's face was somewhat asymmetrical as to the orbits, the left orbit being

higher in proportion to its breadth than the right. Both had a higher index than the Whitechapel mean male skull, but while the difference is not significant in the case of the right orbit, it seems to be otherwise with the left. We see from the figures that this apparent significance is largely due to the fact that the standard deviation for the left orbital index of the Whitechapel males is considerably less than for the right. If the latter had been the appropriate one by which to measure the larger of Sir Thomas Browne's orbital index deviations, we should have found the chances of this index being equalled, or exceeded, as many as 1 in 21 instead of 1 in 50; and we should have accordingly considered the deviation insignificant. Are we thus to regard greater variation in the right than in the left orbit a characteristic of the total population to which the Whitechapel skulls belong? It might be so, of course, but unless we have strong evidence to the contrary we are inclined to expect the variation to be of the same order for both. If the females of this series had manifested this characteristic too, the evidence would have been strong. As it is, we suspect that it may not have been so true of the total male population as of the 63 skulls whose left orbits were measured; and until further evidence is available, we find it advisable not to stress too much the apparent unusualness of the proportions of the left orbit of Sir Thomas Browne.

We have now exhausted, in our survey, the data with which Macdonell supplied us; but we have another source of information to which we can turn. In his study of Cranial Type-contours, R. Crewdson Benington made use of the Whitechapel and Moorfields material to build up the type-contours of the seventeenth century male Londoner*. The figures on which these contours were based were not published, but they have been preserved in the archives of the Biometric Laboratory, University College, London, and among them we can find the standard deviations of the various measurements used. We thus have a means of judging the importance of any difference which may strike us when we compare Sir Thomas Browne's contours each with its corresponding type.

Fewer points were plotted in the construction of these type-contours than it is the custom to take now: in the vertical one, for instance, the crests of the zygomatic ridges were not marked, nor were the temporal ridges in the horizontal type-contour. The omission of these features modifies somewhat the appearance of the type-contours which Benington gave us, but we can allow for such differences. Again, in the sagittal type-contour, only that portion which lay above the horizontal through the nasion (the $N\gamma$ line) was plotted. To this has now been added the auricular point, the co-ordinates of which, as estimated by Mr Geoffrey Morant, are: length of ordinate to $N\gamma$ line, 30·0 mm.; length of abscissa measured from γ along $N\gamma$, 93·7 mm.; S.D. of abscissa, 5·86 mm. These figures are based on measurements taken on the same contours as Crewdson Benington used for his sagittal type-contours, viz. 78 of the Whitechapel male contours and 20 of the Moorfields. The other sagittal contours of the two series were too incomplete to allow of the standard horizontal plane being determined.

* See *Biometrika*, Vol. VIII. pp. 143—147, 191—195. We regret that space has not permitted the reproduction of the contours here.

For the other two type-contours, the horizontal and vertical, Benington used only Whitechapel skulls: 113 for the former, 65 for the latter.

Comparing Sir Thomas Browne's three contours with these, we will begin with the sagittal, and will place nasion on nasion, and $N\gamma$ along $N\gamma$. Of course the small height of Sir Thomas Browne's skull, which was already noted in our figures, becomes at once very apparent. But what is further revealed is the fact that the deficiency in auricular height can be attributed entirely to that part which lies above the level of the nasion. The auricular points of the two contours very nearly coincide; their distance from the $N\gamma$ line is thus practically the same, the slight difference of 2 mm. being in fact in Sir Thomas Browne's favour. This gives us 70·6 mm. for the portion of the auricular height lying above $N\gamma$ in Sir Thomas Browne's contour, against 82·1 in the type-contour.

If, while keeping nasion upon nasion, we revolve one contour a little until the bregmas coincide as nearly as possible, we see that the curve of the frontal bone is very much the same in both cases. Sir Thomas Browne's low forehead is therefore not due to a flattened frontal bone, but to one which is rotated further backwards. The degree of rotation may be measured by the bregmatic angle, i.e. the angle between $N\beta$ and $N\gamma$. For Sir Thomas Browne, this is 38·2°; for the type, 47·1°. Measured by the appropriate S.D., 3·36°*, this or a greater degree of rotation of the frontal is seen to be likely to occur in only 1 in 248 of the population.

If we compare the bregma to lambda curves also, by superposition, we see that their convexity is practically the same, and their length differs little. We cannot compare the curve of the occipitals in the same way with our incomplete type-contour, but we have already seen from the occipital index (*Oc. I.*, Table II B) that there is no significant difference here. The difference between the two sagittal contours, Sir Thomas Browne's and the type, is due therefore much more to the different positions of the bones than to difference in shape.

Let us now restore the contours to their first position ($N\gamma$ lines together). We notice that the greater length of Sir Thomas Browne's skull is due to an increase in the length of that portion only which lies behind the vertical through the auricular points. The length of this portion of $N\gamma$ is 93·7 mm. in the type, 103·7 mm. in Sir Thomas, giving an excess of 10 mm. for the latter. We must of course express this difference in terms of the S.D. in order to get a measure of its significance. The S.D. is 5·86 mm.; Sir Thomas Browne's deviation is 1·71 times the S.D., and the expectation of an excess as large as this is 1 in 23 of the population. The difference noted is, therefore, not very significant.

Turning now to the vertical contours, we note only one outstanding divergence from the type, and that is one we have already dealt with elsewhere, namely, the deficiency in auricular height. We pass on therefore to the horizontal contours. Here we note that although, as we have learnt from the direct measurements, Sir Thomas Browne's frontal breadth is unusually small, his skull widens out very quickly, so that it equals the type-skull in breadth by the time Ordinate 3 of the

* Calculated by Mr G. Morant.

contour is reached. Where the maximum breadth occurs, between ordinates 6 and 7, his measurement is somewhat the larger, although, as measurement B shewed, not significantly larger. We notice, however, from the contours that the outlines to left and right, in the type, converge more quickly after the maximum is passed. It was doubtless partly owing to this characteristic that Mr Robert Fitch described "the back part" of Sir Thomas' skull as "exhibiting a remarkable degree of depth and capaciousness." It would be of interest therefore to get some measure of the width of the back part, and for this we may avail ourselves of the constants worked out for Ordinates 9 and 10 in the type. From the S.D.'s (as given by Benington) of the left and right portions of these ordinates, I have calculated the S.D.'s of the width of skull at these points. The results are as follows:

	Ordinate 9 ($L+R$)	Ordinate 10 ($L+R$)
Sir T. B.	129·6 mm.	102·9 mm.
Type contour	117·4 „	88·6 „
Excess of Sir T. B. over Type ...	12·2 „	14·3 „
S.D. of character	4·54 „	3·78 „
Excess in terms of S.D.	2·69	3·78
Expected frequency in population ...	1 in 280	1 in 12755

Thus while the width at the horizon of Ordinate 9 is unusual, the extent to which it is maintained at Ordinate 10 is still more unusual. In this character Sir Thomas Browne differed, not by defect, but by excess, from the average Londoner of his day.

With the material now examined we come to an end of all available comparative data which are accompanied by figures enabling us to gauge the significance of any deviations we may observe. At this point, therefore, it will be well to sum up the results of our comparisons.

Taking the constants obtained from the Whitechapel series of skulls as the nearest approximation we can get to the cranial constants of the population of London in the seventeenth century, and supplementing these constants, where they are lacking, by data obtained from the Moorfields series, we gather that Sir Thomas Browne's skull possessed the following characteristics: It was quite normal as regards length and maximum breadth, but was deficient in height, this deficiency occurring in the height of the vault above the horizon of the nasion. Its circumference, however, whether horizontal, vertical or sagittal measurements be taken, is quite normal. Of the major proportions of the skull, those in which height is a factor (height in relation to breadth, and to length) are unusual. The breadth of his forehead is significantly smaller than the average breadth, but the skull widens rapidly at the temples, and having attained normal maximum breadth in the region where the maximum usually occurs (a little more than mid-way from front to back) it maintains more than is usual of this breadth as it approaches the occipital region. The breadth at the back of the head is thus significantly greater than usual, and, the length of the head behind the vertical through the auricular passages having also an excess over the normal which borders on significance, the small

capacity of the skull in the frontal region is compensated for by increased capaciousness at the back, with the result that the total capacity of the skull comes within the normal range. The face presents no unusual features with the possible (but not probable) exception of the proportions of the right orbit.

It is unfortunate that we are unable to deal with the mandible in our study, but figures for a series of certainly male English mandibles are at present lacking.

(VIII) The Endocranial Cast of Sir Thomas Browne's Skull.

At the request of Professor Karl Pearson, Professor G. Elliot Smith most kindly consented to write a brief report on the endocranial cast of Sir Thomas Browne.

Report. The general features, so far as the shape and size of the brain are concerned, follow closely the conclusions which have been already set forth with reference to the skull. The special features of the endocranial cast can be best elucidated by means of orthographic projections of the cast compared with those of other endocranial casts (Plates XXXII—XXXIV). This has been done in the three sets of superimposed contours representing the endocranial cast of Sir Thomas Browne's skull in contrast with that of Dean Swift's, which is of large size and shows exceptional development, and also one, obtained from a modern English skull picked at random from the teaching collection in the Anatomical Department of University College, the capacity of which is 1463 c.c. The random specimen of English skull is somewhat narrower and a little higher than the average of the normal series, but it serves to bring out the flatness of Sir Thomas Browne's cast, most pronounced in the frontal region. The outstanding peculiarity of the cast, in fact, is this lowness and the next feature is the marked depression of the prefrontal region of the brain, which is the area which modern research seems to suggest as the part of the brain the high development of which is intimately correlated with powers of concentration and discrimination. In contradistinction to this, if one takes into account its unusual breadth, the temporal area of the brain is well-developed and the parietal area is also larger than the average.

Comparison with the endocranial cast of Dean Swift's skull shows the very definite superiority of the latter in every region. The normal asymmetry is not pronounced. The prominence of the anterior end of the right hemisphere and of the posterior end of the left and the larger size of the right sinus are conditions usual in right-handed persons. These features are less pronounced than is normally the case. There is nothing in the peculiar features of the cast indicative of exceptional or particular development of brain.

(IX) Concluding Remarks.

The general conclusions to be drawn from this discussion of the skull and portraits of Sir Thomas Browne are, we think, of the following nature:

(*a*) There are two original portraits of Sir Thomas Browne only, namely: the Buccleuch Miniature and the Norwich Painting. It is not certain, however, that they are absolutely independent. Of these the miniature is the later, and may be to some extent dependent on the painting.

(*b*) The skull recently re-interred is undoubtedly the same as was seen and reported on by Mr Fitch in 1840. That gentleman appropriated the coffin plate, and may well have appropriated the skull. The skull is the same as that of which a cast was presented to the Norwich Museum in 1841 as Sir Thomas Browne's.

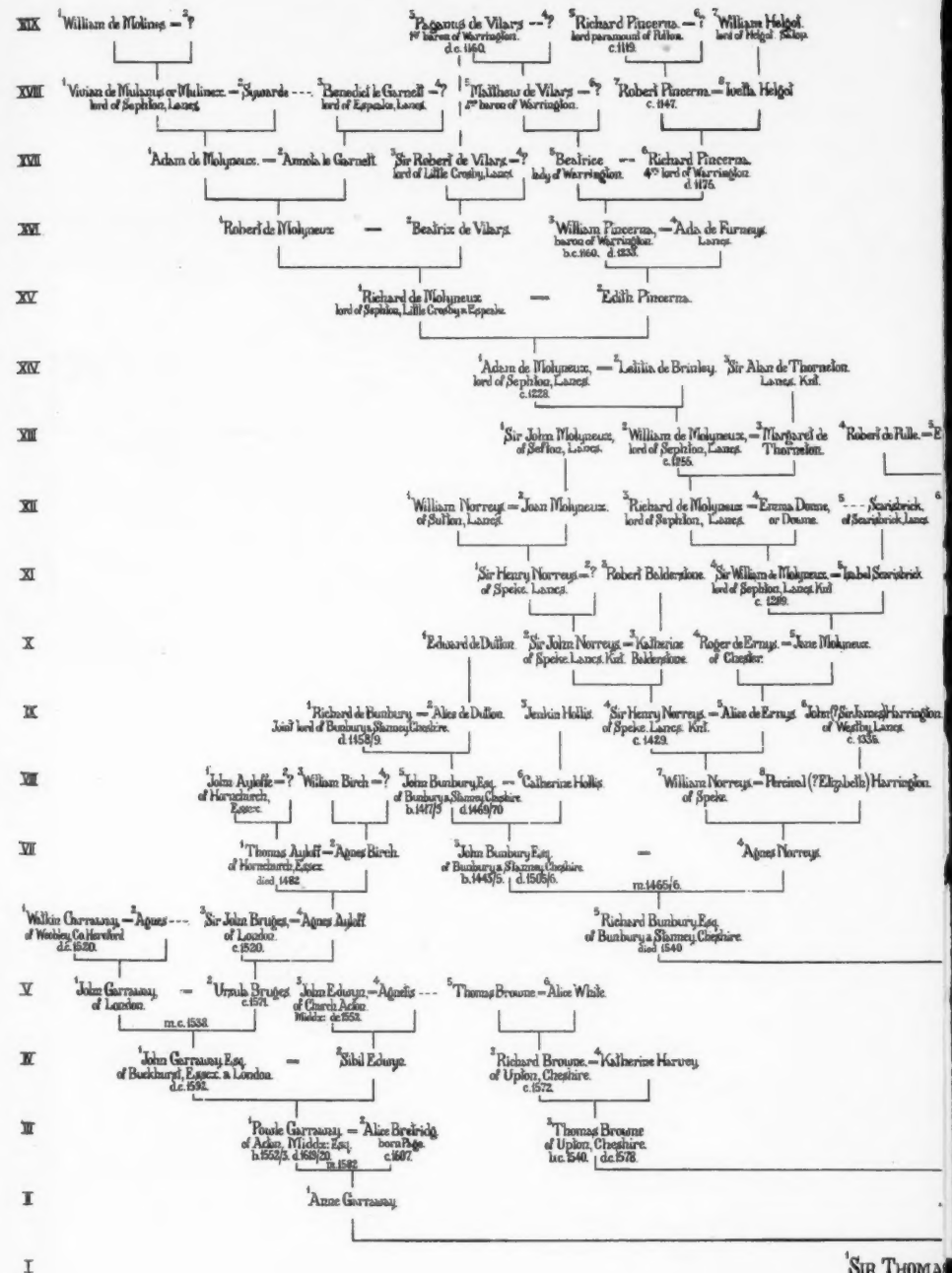
(*c*) The Norwich painting and the Buccleuch miniature are as close to the form that in life draped the skull as we can reasonably expect such portraits to be. They indicate, if they do not stress, the markedly receding frontal of the skull. This not very flattering characteristic, apparently inherited from his father by Dr Edward Browne, the son of Sir Thomas, is wholly removed in the portraits in the Royal College of Physicians and at the Bodleian, but neither of these pictures is an original. It is more or less disguised in the plumbago drawing and in White's engraving.

(*d*) This outstanding peculiarity of the skull, emphasised also in the endocranial cast, seems to suggest a defect of marked development in that part of the brain intimately correlated with powers of concentration and discrimination. There are two possible solutions of the problem raised by this result:

(i) Sir Thomas Browne is undoubtedly a characteristic writer who has charmed many generations of English readers; but after all do his writings suggest great powers of concentration or discrimination? He was in the first place a lover of literature and not a scientist; he was never elected a fellow of the Royal Society, and he did not hesitate to make a public avowal of his belief in witchcraft. We should rather anticipate that the sense of rhythm, the appreciation of sound and music, the artistic rather than the logical side of mind would be markedly developed in him. Hence it is possibly not reasonable to demand that Sir Thomas Browne must have been "high browed." Still the receding frontal is not what some admirers of Sir Thomas Browne may demand for their hero, and to such the Royal College of Physicians or the Bodleian portrait will appeal as satisfying not only their demand but also popular opinion as to the relation of intelligence and head shape.

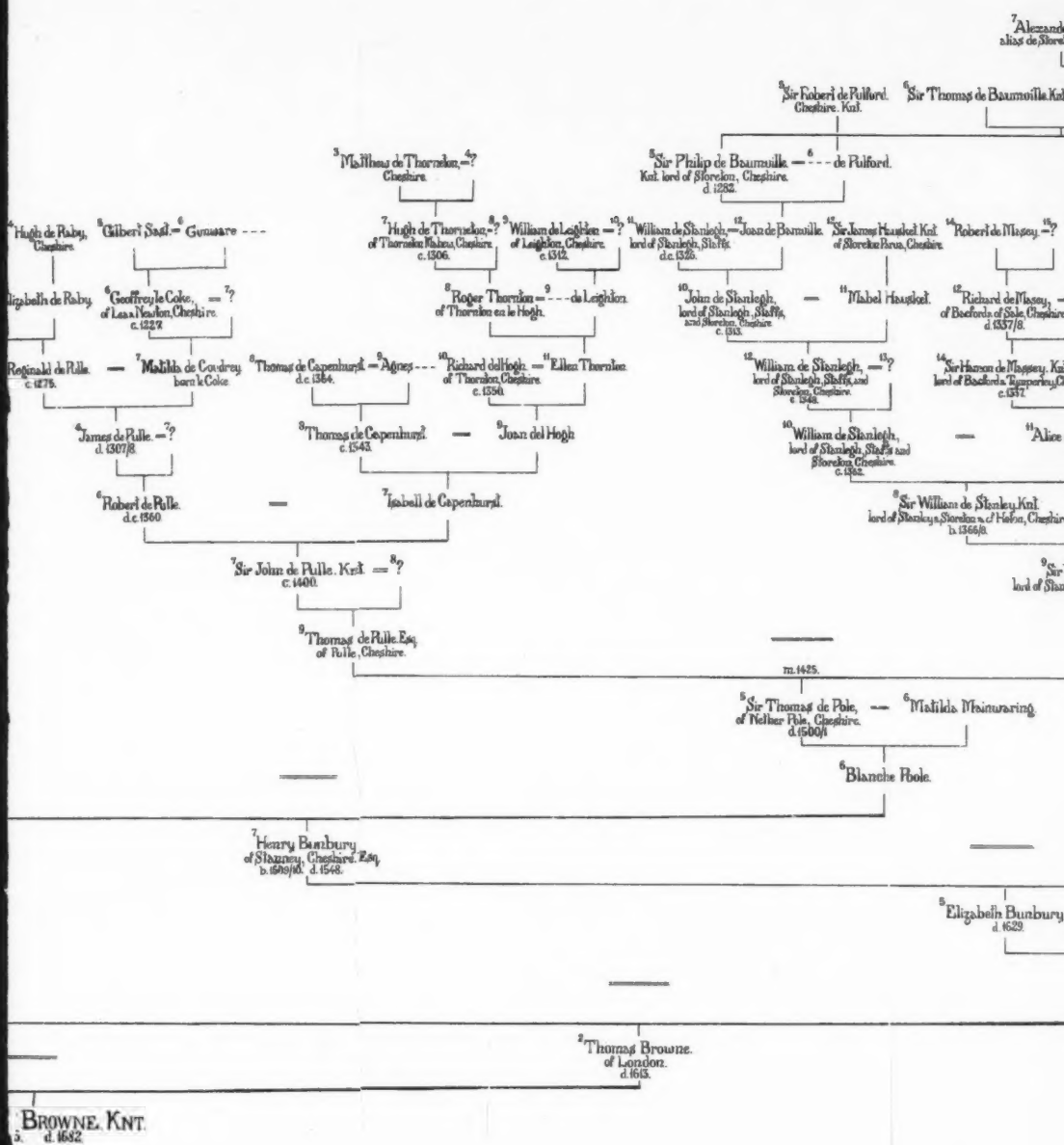
(ii) The other solution of the problem is to take the results of this memoir as confirming earlier investigations which indicate that there is very little correlation between the shape of the head or indeed of the brain cavity and the mentality of the individual. Readers of *Biometrika* may remember that some years ago an investigation of the skull of Jeremy Bentham* showed that it was essentially mediocre with no outstanding characters which might be associated with marked intellectuality. The present investigation seems to indicate that the skull of another man of genius can depart from general mediocrity in a few isolated characters, and in some of these reach a form which current opinion describes as a "low type" of skull. Thus the second solution, and to us the more reasonable one, may be summed up in the words that Sir Thomas Browne's skull supports the conclusion that the correlation of superficial head and brain characters with mentality is so low as to provide no basis for any prognosis of value.

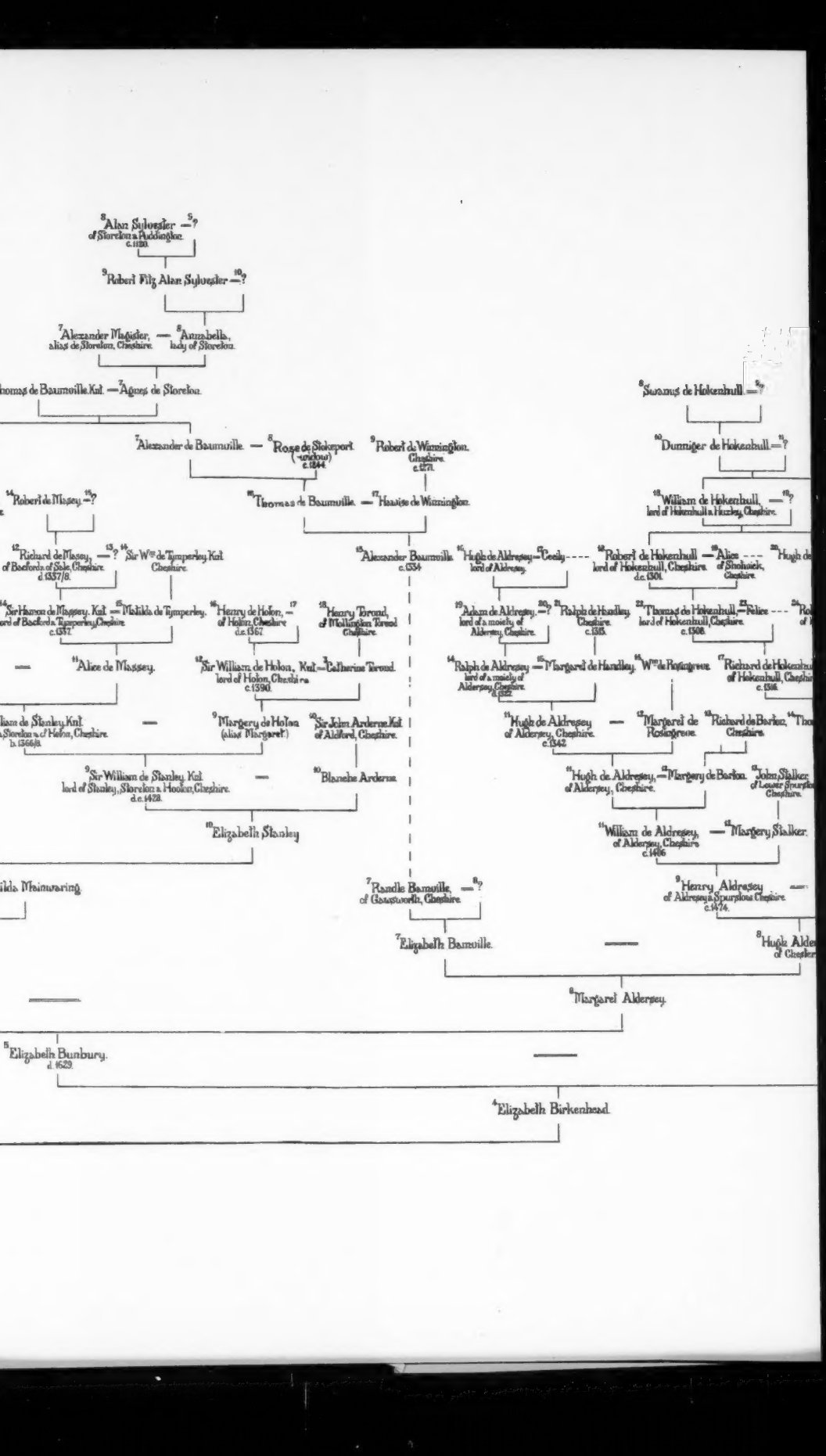
* *Biometrika*, Vol. III. p. 393.

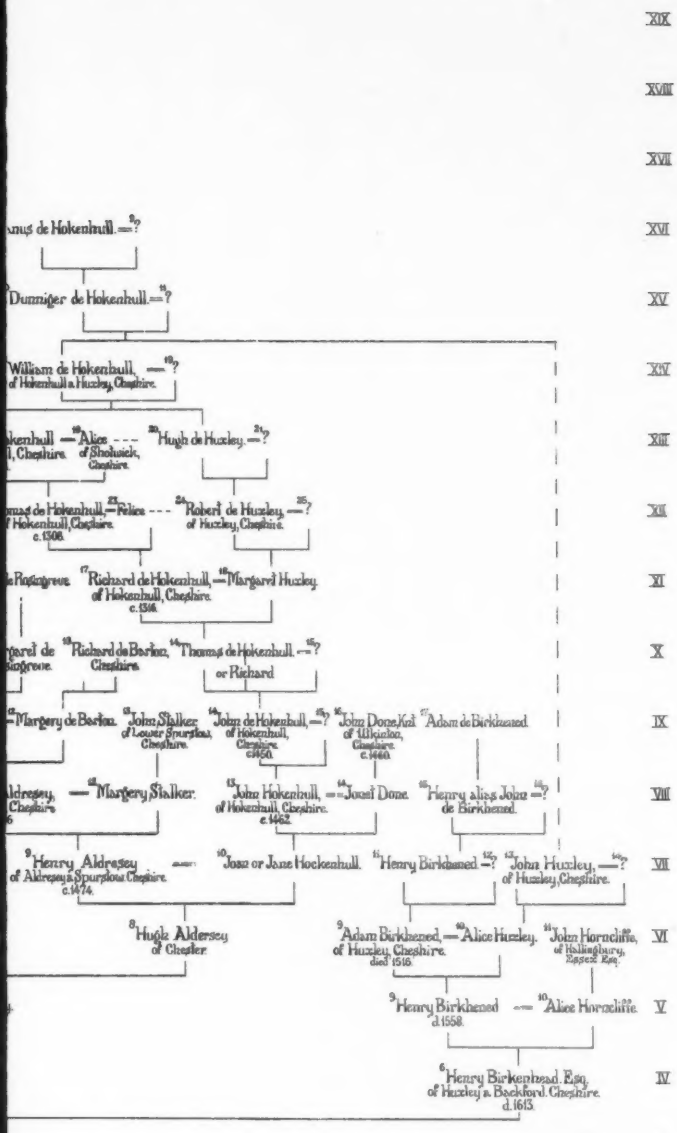

XIX
1William de Molines — 2?
5Paganus de Vilars — 4? 1st baron of Warrington. d.c. 1160.
5Richard Pincerna — 6? lord paramount of Rilton. c.1119.
7William Helgel lord of Helgel Castle.
XVIII
1Vivian de Malvayne or Malineux — 2Synarda
Benedict le Garnett — ? lord of Esprake, Lancs.
5Matthew de Vilars — 6? 2nd baron of Warrington.
7Robert Pincerna — Isella Helgel c. 1147.
XVII
1Adam de Molyneux. — 2Amabel le Garnett
3Sir Robert de Vilars — 4? lord of Little Crosby, Lancs.
5Beatrice — lady of Warrington.
6Richard Pincerna. 4th lord of Warrington. d. 1175.
XVI
1Robert de Molyneux — 2Beatrix de Vilars.
3William Pincerna, — 4Ada de Furnays baron of Warrington. b.c.1160. d.1233. Lancs.
XV
1Richard de Molyneux lord of Sephton, Little Crosby & Esprake. — 2Edith Pincerna.
XIV
1Adam de Molyneux. — 2Letitia de Brimley. 3Sir Alan de Thornton. Lancs. Knt.
c.1228.
XIII
1Sir John Molyneux, of Sefton, Lancs. 2William de Molyneux, — 3Margaret de Thornton 4Robert de Pulle. — 5
lord of Sephton, Lancs. c.1255.
XII
1William Norreys — 2Joan Molyneux. 3Richard de Molyneux — 4Emma Dunne, — 5 — Scarisbrick, 6
of Sefton, Lancs. lord of Sephton, Lancs. or Dunne. of Scarisbrick, Lancs.
XI
1Sir Henry Norreys — ? 2Robert Balderston 3Sir William de Molyneux — 4Isabel Scarisbrick
of Speke, Lancs. lord of Sephton, Lancs. Knt c. 1289.
X
1Edward de Dutton 2Sir John Norreys — 3Katherine 4Roger de Ermys. — 5Anne Molyneux.
of Speke, Lancs. Knt. Balderston of Chester.
IX
1Richard de Bunbury — 2Alice de Dutton. 3Jenkin Hollis 4Sir Henry Norreys. — 5Alice de Ermys 6John (? Sir James) Harrington
Lord lord of Bunbury, Stanney, Cheshire. of Speke. Lancs. Knt. of Wedby, Lancs.
d.1458/9. c.1429. c.1356.
VIII
1John Aukiffe — ? 2William Birch — ? 3John Bunbury, Esq. — 4Catherine Hollis 7William Norreys — 8Percival (? Elizabeth) Harrington
of Horndurch, Essex. of Bunbury, Stanney, Cheshire. of Speke.
b.1417/8 d.1469/70
VII
1Thomas Aukiff — 2Agnes Birch. 3John Bunbury Esq. — 4Agnes Norreys.
of Horndurch, Essex. of Bunbury, Stanney, Cheshire.
died 1482. b.1443/5. d.1505/6. m.1465/6.
1Walkin Garraway — 2Agnes — 3Sir John Bruges — 4Agnes Aukiff 5Richard Bunbury, Esq.
of Wooley, Co Hereford of London. of London. of Bunbury, Stanney, Cheshire.
d.c.1520. c.1520. died 1540
V
1John Garraway, — 2Ursula Bruges 3John Edwyn — 4Agnes — 5Thomas Browne — 6Alice Wisle.
of London. c.1571. of Church Aston. Widdr. de 1553.
m.c.1538.
IV
1John Garraway Esq — 2Sibil Edwyn. 3Richard Browne. — 4Katherine Harvey.
of Buckhurst, Essex. & London. of Upton, Cheshire.
d.c.1592. c.1572.
III
1Powle Garraway — 2Alice Bredrick 3Thomas Browne
of Acton, Middx. Esq. born Pope. of Upton, Cheshire.
b.1552/3. d.1618/20. c.1607. b.c.1540. d.c.1578.
m.1582
II
1Anne Garraway.
I
1Sir Thomas
b. 160

PEDIGREE OF SIR THOMAS BROWNE.

AS FAR AS KNOWN AT PRESENT.


[7]Alexand
alias de Store
[8]Sir Robert de Pulford. Cheshire. Knt.
[6]Sir Thomas de Baumoille. Knt
[3]Matthew de Thornton. =?
Cheshire.
[5]Sir Philip de Baumoille. = - - - de Pulford.
Knt. lord of Storeton, Cheshire.
d. 1282.
[4]Hugh de Raby, Cheshire.
[5]Gilbert Sadl. = Gunnare - - -
[7]Hugh de Thornton = ? [8, 9]William de Leighton = ? [11]William de Stanleigh. = Joan de Baumille. [12]Sir James Hauskel Knt. [14]Robert de Massey. = ?
of Thornton Hahna, Cheshire. of Leighton, Cheshire. lord of Stanleigh, Staffs. of Storeton Parva, Cheshire.
c. 1306. c. 1312. d.c. 1326.
Elizabeth de Raby. [6]Geoffrey le Coke. = ?
of Lea x Newton, Cheshire.
c. 1227
[8]Roger Thornton = - - - [9]de Leighton.
of Thornton en le Hogh.
[10]John de Stanleigh. — [11]Mabel Hauskel. [12]Richard de Massey. =
lord of Stanleigh, Staffs, and Storeton, Cheshire. of Beeford a. Sale, Cheshire
c. 1313. d. 1357/8.
Reginald de Pulle. — [7]Matilda de Coudrey born le Coke
c. 1275.
[12]William de Stanleigh. = ? [14]Sir Hamon de Massey. Kn
lord of Stanleigh, Staffs, and lord of Beeford a. Tiggeriley, C
Storeton, Cheshire. c. 1337.
c. 1348.
[6]James de Pulle. = ? [8]Thomas de Capenhurst. = Agnes - - - [10]Richard del Hogh. = [11]Ellen Thornton
d. 1307/8. d.c. 1364. of Thornton, Cheshire.
c. 1350.
[8]Thomas de Capenhurst. — [9]Joan del Hogh [10]William de Stanleigh, — [11]Alice
c. 1543. lord of Stanleigh, Staffs and
Storeton, Cheshire.
c. 1382.
[6]Robert de Pulle. — [7]Isabell de Capenhurst.
d.c. 1360
[8]Sir William de Stanley, Knt.
lord of Stanley a. Storeton a. of Hooton, Cheshir
b. 1366/8.
[7]Sir John de Pulle. Knt. = [8]?
c. 1400.
[9]Thomas de Pulle. Esq.
of Pulle, Cheshire.
m. 1425.
[5]Sir Thomas de Pole, — [6]Matilda Mainwaring.
of Nether Pole, Cheshire.
d. 1500/1
[6]Blanche Poole.
[7]Henry Bunbury
of Stanney, Cheshire. Esq.
b. 1509/10. d. 1548.
[5]Elizabeth Bunbury
d. 1629.
[2]Thomas Browne.
of London.
d. 1613.
BROWNE. KNT.
d. 1682.


8 Alan Sylvester — 5 ?
of Storeton a Puddinglow.
c.1120.
9 Robert Fitz Alan Sylvester — 10 ?
7 Alexander Magister, alias de Storeton, Cheshire. — 8 Annabella, lady of Storeton.
homas de Baumville Knt. — 7 Agnes de Storeton.
7 Alexander de Baumville. — 8 Rose de Stokeport (widow) c.1244
9 Robert de Winnington. Cheshire. c.1271.
14 Robert de Massey. — 15 ?
16 Thomas de Baumville. — 17 Hawise de Winnington.
12 Richard de Massey, of Basford in Sale, Cheshire. d.1357/8. — 13 ?
14 Sir Wm de Timperley Knt. Cheshire.
15 Alexander Baumville. c.1334
16 Hugh de Aldresey — Cecily
17 lord of Aldresey.
14 Sir Hamon de Massey, Knt. — 15 Matilda de Timperley.
lord of Basford a Timperley, Cheshire. c.1317.
16 Henry de Holon, — 17 of Holon, Cheshire d.c.1367.
18 Henry Torond, of Mollington Torond Cheshire.
19 Adam de Aldresey. — 20 ?
lord of a moiety of Aldersey, Cheshire.
21 Ralph de Hardley, Cheshire. c.1315.
22 Thomas de Hokenhull. — 23 Felice
lord of Hokenhull, Cheshire. c.1308.
— 11 Alice de Massey.
12 Sir William de Holon, Knt. — 13 Catharine Torond.
lord of Holon, Cheshire c.1390.
14 Ralph de Aldresey — 15 Margaret de Hardley.
lord of a moiety of Aldersey, Cheshire. d.1322.
16 Wm de Poynegrave.
17 Richard de Hokenhull of Hokenhull, Cheshire c.1316.
liam de Stanley Knt. — 9 Margery de Holon (alias Margaret) — 10 Sir John Arderne Knt. of Aldford, Cheshire.
Storeton a of Holon, Cheshire. b.1366/8.
11 Hugh de Aldresey of Aldersey, Cheshire. c.1342 — 12 Margaret de Rosingrave. — 13 Richard de Barlow, Cheshire.
9 Sir William de Stanley Knt. lord of Stanley, Storeton a Hooton, Cheshire. d.c.1428. — 10 Blanche Arderne.
11 Hugh de Aldresey, of Aldersey, Cheshire. — 12 Margery de Barlow.
10 Elizabeth Stanley
11 William de Aldresey, of Aldersey, Cheshire c.1406 — 12 Margery Stalker.
ilda Mainwaring.
7 Randle Bamville, of Gousworth, Cheshire. — 8 ?
9 Henry Aldresey of Aldresey a Spurstow Cheshire. c.1474.
7 Elizabeth Bamville.
8 Margaret Aldersey.
5 Elizabeth Bunbury. d.1629.
4 Elizabeth Birkenhead.


XIX
XVIII
XVII
XVI
XV
XIV
XIII
XII
XI
X
IX
VIII
VII
VI
V
IV
III
II
I
Hokenhull.=?
Hokenhull.=?
William de Hokenhull, =?
of Hokenhull a Huxley, Cheshire.
Alice
of Shotwick, Cheshire.
Hugh de Huxley.=?
Felice
Robert de Huxley, =?
of Huxley, Cheshire.
Richard de Hokenhull, =Margaret Huxley
of Hokenhull, Cheshire.
c. 1316.
Richard de Barlow,
Cheshire.
Thomas de Hokenhull. =?
or Richard
Margery de Barlow.
John Stalker,
of Lower Spurstow, Cheshire.
John de Hokenhull, =?
of Hokenhull, Cheshire.
c.1450.
John Done, Knt
of Utkinton, Cheshire.
c.1440.
Adam de Birkinead.
Margery Stalker.
John Hokenhull, =Josel Done.
of Hokenhull, Cheshire.
c.1462.
Henry alias John =?
de Birkinead.
Henry Aldersey =
of Aldersey & Spurstow Cheshire.
c.1474.
Joan or Jane Hockenhull.
Henry Birkinead.=?
John Huxley, =?
of Huxley, Cheshire.
Hugh Aldersey
of Chester.
Adam Birkinead, =Alice Huxley.
of Huxley, Cheshire.
died 1516.
John Horncliffe,
of Hollingbury, Sussex Esq.
Henry Birkinead =Alice Horncliffe.
d.1558.
Henry Birkenhead, Esq.
of Huxley a. Beckford, Cheshire.
d.1613.

APPENDIX I.

Notes on Pedigree.

II 2. Mercer, of Cheapside, London.

III 1. Baptized at Acton 20 Feb. 1552/53; married (1) at St Peter-le-Poer, London, May 1582, to Alice Bretridg, widow, (2) at Tingrith, Beds. Aug. 1610, to Margaret Brydiman, widow; buried at St Martin-in-the-Fields, London, 24 Jan. 1619/20, "as Poll Garaway of Tingriffe, Co. Beds. Esq^re^"; admon. to son Phillip, P.C.C. 23 Feb. 1619/20.

III 2. Widow of George Bretridg of London and Iver, Co. Bucks.; living 10 Nov. 1607 (date of Barthol. Garraway's will).

III 3. Born circa 1540; will dated 15 May 1572; proved 12 Oct. 1578.

IV 1. Admon. in P.C.C. 6 Dec. 1592, to son Powle; therein described as "of Acton, Co. Middx."

IV 2. Widow of [John] Lain of the Privy Chamber Edw. VI; married to John Garraway between 17 Aug. 1545 and 1 Mar. 1552/53 (will of Jno. Edwyn and admon. to Agnes Edwyn).

IV 3, IV 4. Both living 1572.

IV 5. Died 12 Aug. 1629; 2nd wife.

IV 6. Prothonotary and Clerk of the Crown for the counties of Chester and Flint; purchased the manor of Backford June 1571, tog. with estates in Bromborough from Thos. Aldersey, haberdasher; died 7 Mar. 10 Jac. I (1613); Inq. p.m. 12 Jan. 11 Jac. I (1614).

V 1. Married before 1538, a younger son.

V 2. Living 18 Dec. 1571 (will of son Walter).

V 3. Will dated 17 Aug. 1545; admon. granted to wife 1 Mar. 1552/53.

V 5. *Brownes of Upton.* Until the dissolution of the monasteries the Brownes of Upton were fee-farmers, in fee to Chester Abbey; the lands were thereafter confirmed to them. (Ormerod's *Hist. of County of Chester*, Vol. II. p. 819).

V 7. Born 1 Hen. VIII (1509–10), died 8 Jan. 38 Hen. VIII (1547).

V 8. Heiress of father; m. (1) Hy. Bunbury, (2) Sir Rowland Stanley of Hooton, Knt. descendant of IX 9.

V 9. 2nd son; Clerk of the Green Cloth to Hen. VIII, Prothonotary and Clerk of the Crown for the counties of Chester and Flint.

V 10. Heiress of father.

VI 1. Will 16 Mar. 1518/19; proved by wife P.C.C. 10 May 1520 (26 Ayloffe); brass in Church of the Apostles Peter and Paul, Weobley, where buried.

The Garways or Garraways were an old Herefordshire family who until the 16th cent. had lands at Weobley; these were then added to the adjacent estate of the Ley, the seat of a branch of the Brydges family (Ch. J. Robinson's *Mansions and Manors of Herefordshire*, p. 290).

VI 3. Citizen, draper, and alderman of London; sheriff 1513, Lord Mayor 1520.

Brugge (*Brydges, Bruges*) *Pedigree*, Banks' *Dormant and Extinct Peerages*, Vol. III.; line traced from Sir Simon de Brugge (temp. Hen. III and Edw. I) to Simon Brugge of the Leye, Herefordshire, whose will is dated 1385 (Robinson, loc. cit. [VI 1]). This Simon Brugge of the Leye was the ancestor of Sir John Bruges [VI 3] (Banks, loc. cit.). The pedigree is incomplete, but the descent was evidently through a younger son, for there were Brydges of the Ley until 1702 (Robinson, loc. cit. p. 291).

VI 5. Living 22 Hen. VII (1506–7), died 27 Nov. 32 Hen. VIII (1540), seized of manors mentioned IX 1.

VI 8. Alderman of Chester; 3rd son.

VI 9. Died 19 Aug. 1516, buried at St Mary's, in Chester, where a monument remains to his memory.

VI 10. Heiress of father.

VI 11. *Erratum* in pedigree, *for* Hallingbury *read* Hallowbury.

VII 3. Aet. 25, 9 Edw. IV (b. 1443–5); obiit et Inq. p.m. 21 Hen. VII (1505–6); marr. 5 Edw. IV (1465–6).

VII 5. Alias Tho. de Pulle jun., Inq. p.m. 16 Hen. VII (1500–1).

VII 6. Living 1454–5; very probably of Cheshire family: Ormerod gives accounts of Mainwarings of 8 separate places in Cheshire.

VII 7. Descended from the Bamvilles of Chester (see XIII 15).

VII 9. Living, 14 Edw. IV (1474–5).

VII 13. *Huxley Pedigree*, Ormerod, II. 800 (Alice [VI 10] not given).

VIII 1. The ancient seat of the *Ayloff family* was Boughton Aloph in Kent, to which its name was given. Differing accounts of the family are given in Hasted's *Hist. of Kent*, III. 190, and Wright's *History of Essex*, II. 443. The latter states that it was a Saxon family, one of whom was portreeve of London, temp. Edw. Confessor.

VIII 5. Aet. 40, 37 Hen. VI (b. 1417–9); obiit et Inq. p.m. 9 Edw. IV (1469–70), seized of lands mentioned IX 1.

VIII 9. Married by dispensation 1425, being related to his wife in 4th degree; living 1454–5.

VIII 11. Occurs 7 Hen. IV (1405–6) and 25 Hen. VI (1446–7); lord of a moiety of Aldersey.

VIII 12. Heiress of father.

VIII 13. Living 2 Edw. IV (1462–3); appears in a plea to quo warranto 15 Hen. VII (1499–1500); Inq. p.m. 16 Hen. VII (1500–1).

VIII 15. Living temp. Hen. V & VI.

IX 1. Died seized of the manors of Bunbury, Stanney, and Occleston, and of lands in Hoole and divers other townships; living 2 Hen. V (1414–5); obiit et Inq. p.m. 37 Hen. VI (1458–9).

Bunbury Pedigree, Ormerod, II. 395; succession shown through 12 predecessors (relationships uncertain) from...de St Pierre, a younger brother of the house of St Pierre in Normandy.

IX 3. Of Flint.

IX 4. A juror 9 Mar. 7 Hen. VI (1429).

IX 6. The date on pedigree c. 1335 is an *erratum*. *Harrington Pedigree*, Baines' *History of County of Lancs.* Vol. v. p. 74; goes back to Robert de Haverington (or Haryngton) Co. Cumberland, and lord of Aldingham, Co. Lancs.; summoned to Parliament 1324, 1326, 1327, 1349.

IX 7. In the Scotch and Irish wars, temp. Ric. II; governor of Carnarvon Castle 1 Hen. IV (1399–1400).

IX 9. Pardon for Percy rebellion 3 Nov. 5 Hen. IV (1403) at the battle of Agyncourt; knighted 1415; living 4 Hen. VI (1425–6); died before 7 Hen. VI (1428–9).

IX 12. Heiress of father.

IX 14. Made a settlement of the manor of Hokenhull 28 & 31 Hen. VI (1449–50, 1452–3).

IX 16. *Done Pedigree*, Ormerod, II. 248 (Jonet [VIII 14] not given). The first of the Dones who held Utkinton (under the barons of Kinderton) was Richard Done, temp. K. John. IX 16 is probably the Sir John Done described in the pedigree as "Inq. p.m. 38 Hen. VI (1459–60), Knt."

IX 17. *Birkenhead Pedigree*, Ormerod, II. 368. Three predecessors of IX 17 given (relationships uncertain) of whom the first was John de Birchened, temp. Edw. II & III.

X 1. *Dutton Pedigree*. Ormerod, II. 795–6 (Alice [IX 2] not given); goes back to Wlofaith Fitz-Ivon, temp. Will^{m} I.

X 5. Heiress of father. See note to XII 2.

X 6. Died between 24 & 42 Edw. III (1350–69).

X 7. Heiress of father.

X 8. Lord of Hoton, Cheshire, in right of his wife; in Ireland on King's service 1 Hen. IV (1399–1400); pardon for Percy rebellion 3 Nov. 5 Hen. IV (1403) at the battle of Agyncourt; aet. 30, 21 Ric. II (b. 1366–8); Inq. p.m. 6 Hen. VI (1427–8).

X 9. Sole heiress of father.

X 10. *Arderne Pedigree*, Ormerod, II. 85 (Blanche [IX 10] not given); line goes back to Agnes de Arden (born 11th cent.), ? descended from Turketill de Eardene, Co. Warwick.

X 11. Lord of a moiety of Aldersey; occurs 16 Edw. III (1342–3).

X 13. *Barton Pedigree* (incomplete), Ormerod, II. 749 (Margery, died 1355, [IX 12] not given); goes back to Sir Patrick Barton, Knt., lord of Barton, Cheshire, temp. Hen. III.

X 14. Uncertain whether Thomas de Hokenhull was father of John [IX 14], or grandfather, the father then being Richard.

XI 3. *Osbaldeston* (*Balderstone*) *Pedigree*, Baines, IV. 56½ (Katherine [X 3] not given); line goes back to Hugo de Osbaldeston, als. de Sunderland, living temp. Ric. I and John, of Saxon descent (Baines, IV. 55).

XI 4. Made a knight banneret in Gascony by Edmund Crouchback, Earl of Lancaster, in 1289.

XI 6. ?obiit 1 Edw. II (1307–8); grantee of lands in Capenhurst, Cheshire, from Hugh de Bernston.

XI 8, XI 9. Living 17 Edw. III (1343–4).

XI 10. Grantee of 20 marks per annum in lieu of the forestership of Wirral, 35 Edw. III (1361–2); occurs 1 & 2 Ric. II; Inq. p.m. 21 Ric. II (1397–8).

XI 12. Temp. Edw. III; ?in the French wars; protection 1377; Inq. p.m. 20 Ric. II (1396–7).

XI 13. Sole heiress of father.

XI 14. Occurs 18–19 Edw. II (1324–6); died 1327.

XI 17. Mentioned in Plea Rolls 10 Edw. II (1316–7), and Harleian MSS. No. 1535.

XI 18. Part heiress of father.

XII 1. Seized of Speke, Lancs. in right of his wife (but see XII 2).

Norreys Pedigree, Baines, V. 58; the names, only, of six generations before this in the male line, are given, back to Alane Norreys of Sutton, Lancs. The names do not tally with the details given in Baines, V. 21. Norreys is a Norman-French name. There is mention of the family as early as 1199 (Baines, IV. 291).

XII 2. Said (Baines, V. 58) to have brought the lordship of Speke into the Norreys family. Baines, V. 226 states on the other hand that the lordship of Speke came through Jane Molyneux [X 5] whose daughter [IX 5] married Sir Henry Norreys.

XII 6. Living in the time of Simon the Abbot of St Werburgh, Chester, between 1265 and 1289.

XII 7. Widow of Richard de Coudrey; co-heiress with her sister of the hereditary office of cook to the Abbot of St Werburgh, and of her father's lands.

XII 8. Died before 37 Edw. III (before 1364); bore Gules a chevron between three capons argent.

Capenhurst Pedigree, Ormerod, II. 570; five previous generations given in male line, of whom the first is John de Frodesham, temp. Hen. III. In Frodesham Pedigree, Ormerod, II. 48, the line is taken still further back to Hugh de Frodesham, temp. Hen. II and Ric. I.

XII 9. Married 2ndly Robert Calf of Chester, 36–37 Edw. III (1362–4).

XII 10. Obtained lands in Thornton by fine from Wm de Pensby, 3 Edw. III (1329–30); living 23–24 Edw. III (1349–51). (Ormerod, II. 552.)

XII 11. Heiress of father.

XII 12. Claimant of the master-forestership of Wirral, Cheshire; living 21–22 Edw. III (1347–9).

XII 14. Did fealty 11 Edw. III (1337–8); Inq. p.m. 23 Edw. III (1349–50).

XII 15. Heiress of father.

XII 16. Died after 40 Edw. III (after 1366–7).

Hooton Pedigree, Ormerod, II. 410; two predecessors of XII 16 given (relationships uncertain) of whom the first is Wm de Hoton, lord of Hoton, temp. Hen. III and Edw. I.

XII 18. *Re Torond Pedigree*, Ormerod, II. 378. The earliest record of Toronds of Mollington Torond is 2 Edw. II (1308–9).

XII 19. Second son; inherited only a moiety of estate.

XII 21. *Re Handley Family*, Ormerod, II. 724; earliest mention 7 Edw. II (1313–4).

XII 22. Living 1308.

XII 24. Ancestor of the branch of Huxleys whose estate passed to the Clives of Huxley.

XIII 1. *Molyneux Pedigree*, Baines, v. 226 (Sir John [XIII 1] not given).

XIII 2. Knighted 40 Hen. III (1255).

XIII 4. *Poole Pedigree*, Ormerod, II. 423.

XIII 6. Cook to William Marmion, Abbot of St Werburgh, Chester, 1226–8 (office hereditary and descended with lands to daughters).

XIII 9. Heiress of father.

XIII 10. Master-forester of Wirral, Cheshire (hereditary office); living 6–7 Edw. II (1312–4).

XIII 12. Remainder-man in tail of the manor of Bacford, 8 Edw. II (1314–5); ?lord of Sale, obiit ante 11 Edw. III (1337–8).

XIII 15. Living 1334; ancestor of the Bamvilles of Chester, who held lands in Storeton, Cheshire (see VII 7).

XIII 16. Lord of Aldersey, temp. Hen. III.

Aldersey Pedigree, Ormerod, II. 739. The family is supposed to have descended from Hugh de Altaribus, male ancestor at the Conquest (Ormerod, II. 737).

XIII 18. Confirmed his uncle's donations of lands to Pulton Abbey; Sheriff of Cheshire, 50 Hen. III (1265–6); died circ. 30 Edw. I (1301–2).

XIV 1. In the commission for the perambulation of forests; had a forestership in Lancs. 1228.

XIV 4. *Raby Pedigree*, Ormerod, II. 548 (Elizabeth [XIII 5] not given).

XIV 5, XIV 6. *Pedigree of Geoffrey le Coke*, Ormerod, II. 385.

XIV 7. Held of the Earl of Chester, in capite, the manor of Thorneton Maheu, also a moiety of the township of Pulton Waley for one-fourth a knight's fee; also land in the City of Chester and in Pulton Launcelyne 35 Edw. I (1306–7).

XIV 9. Holding part of Leighton under the paramount lordship of the barons of Montalt, in 6 Edw. II (1312–3). Ormerod, I. 549.

XIV 11. Obiit ante 20 Edw. II (before 1326–7).

Stanley Pedigree, Ormerod, II. 415; relationships before William [XIV 11] uncertain, but 4 (?5) predecessors given of whom the first is Adam de Aldithlegh, alias Audley, Co. Staffs., temp. Hen. I; and of whom the third married an heiress of Stonelegh, als. Stanley, Co. Staffs.

XIV 12. Part heiress of father.

XIV 14. Temp. Hen. III and Edw. I.

Mascy Pedigree, Ormerod, II. 365; father of Robert [XIV 14] uncertain; line goes back to great-grandfather, Sir Hamon de Mascy, 1st known baron of Dunham Mascy and lord of Bacford, Cheshire, temp. Hen. I.

XIV 18. Temp. Hen. III.

XV 3. Living temp. Hen. III; probably lord of Thorneton under the Earl of Chester (Ormerod, II. 553).

XV 5. Master-forester of Wirral, Cheshire; obiit in or ante 1282.

XV 8. Widow of Robert de Stokeport, Cheshire, 29 Hen. III (1244–5).

XV 9. *Winnington Pedigree*, Ormerod, II. 205 (Hawise [XIV 17] not given); earliest of line mentioned is Wm de Wynington; his son Robert was living 56 Hen. III (1271–2) and may be XV 9 father of Hawise.

XV 10. Temp. K. John.

XVI 1. Acquired Little Crosby in right of his wife.

XVI 2. Heiress of father; married temp. K. John.

XVI 3. Born about 1160; in 1196 paid 20 marks to be excused crossing the seas with the army into Normandy; rebuilt Warrington Church; in 1225 appointed a collector of the fifteenth in Lancs.

XVI 5. *Pulford Pedigree*, Ormerod, II. 857.

XVI 7. Part-heiress of father.

XVI 8. Temp. Ric. I.

Hokenhull Pedigree, Ormerod, II. 316.

XVII 1. Gave lands in Melling and Conscough, Lancs. to the Church of the Virgin Mary at Cockersand, sealed with the cross moline and the legend "S. Adami de Molinys."

XVII 2. Heiress of father.

XVII 3. Descendant of Paganus de Vilars [XIX 3] (Baines, v. 230).

XVII 5. Only child and heiress.

XVII 6. 2nd son, heir to his brother; a benefactor to Calk Abbey; 4th lord of Warrington in right of his wife; living 1141; died 23 Hen. II (1176).

XVII 7. Tutor of Randle de Blundeville, 6th Earl of Chester.

XVII 8. Sole daughter and heiress of father.

XVIII 3. *Re Garnetts of Espeake.* "Spec" was held at the time of the Domesday Survey by a Saxon thane Uctred. Shortly afterwards it is recorded that Roger Gerneth (Garnett) gave 2 carucates in Spec (Espeake or Speke) in marriage to Richd. de Mulas. (Baines, v. 57.)

XVIII 5. Gave a carucate of land in Crophill to the canons of Fiskerton on Trent, alias Thurgarton.

XVIII 7. Succeeded his father as Earl's butler; living 1147; gave a moiety of his vill of Pulton to William, 1st Abbot of Combermere, for the founding of an abbey at Pulton, 1153 or 1158.

XIX 1. A Norman noble in the train of the Conqueror, 18th in order on the Roll of Battle Abbey; had a grant from Roger de Poictou, lord of Lancaster, of the manor of Sephton (in which place he fixed his abode) and of the manors of Thornton and Kicurden, to be held by the service of half a knight's fee.

Molyneux Pedigree, Baines, v. 226.

XIX 3. Descended from the family of Vilars of L'Isle Adam, in Normandy; received from Roger de Poictou a grant of Crosby, and had also granted to him Warrington, Walton, and Halsall, Lancs.; gave to the Knights Hospitallers of St John of Jerusalem one carucate of land in Bekaneshou (Becconsall) in alms; died about 1160.

Vilars and Pincerna Pedigree, Baines, IV. 400.

XIX 5. Styled "Pincernarius"; butler to the Earl of Chester; lord paramount of Pulton, Cheshire, at the time of the Domesday Survey; a benefactor to St Werburgh's Abbey, Chester, 1119.

XIX 7. ?Lord of Holegate or Helgot in Coverdale, Salop.

XIX 8. Grantee of Storeton, Puddington, and the master-forestership of Wirral, Cheshire, from Randle Meschines, 3rd Earl of Chester, circa 1120.

Storeton, Stanley and Baumville Pedigree, Ormerod, II. 448.

APPENDIX II.

List of Portraits since 1686.

The following is a list, which attempts to be complete, of all portraits of Sir Thomas Browne published since 1686 (the date of Robert White's engraved portrait) up to the present day, classified according to the portraits upon which they were based:

A. *Reproduction of Buccleuch Miniature.*

Photograph (Plate XLIV) in *The Photographic Historical Portrait Gallery*, publ. by P. and D. Colnaghi, Scott and Co., London, 1860 (photographs by Caldesi, Blanford and Co., letter-press by Amelia B. Edwards).

B. *Reproductions from Royal College of Physicians' portrait.*

(1) Engraving by J. Brown after the drawing by G. P. Harding, publ. separately by M. M. Holloway, Covent Garden, London, Jan. 1, 1849 (details of dress being difficult to distin-

guish in original, Harding has introduced a fur-edged cloak similar to that worn by Linacre in Holbein's portrait (painted 1521), of which there is a copy at the Royal College of Physicians).

(2) Same engraving as (1), in *Christian Morals*, publ. by Rivingtons, London, 1863.

(3) Same engraving as (1), in *Portraits of Illustrious Persons in English History*, drawn by G. P. Harding (letterpress by Thos. Moule), publ. by John Russell Smith, London, 1869.

(4) Engraving by C. H. Jeens, in *Religio Medici*, etc., publ. by Macmillan and Co., London, 1881 [cloak copied from G. P. Harding, see (1)].

(5) Lithograph reproduction "after J. Brown" [see (1)] in Harper's *Monthly Magazine* (Vol. LXV. p. 396), 1882, publ. by Harper Bros., New York.

(6) Same as (3) in a reprint of same, 1898.

(7) Portrait said by Mr Ch. Williams to be "Engraving by C. H. Jeens, 1885"; probably publ. separately; cloak plain, and buttoned in front (copy in Norwich Castle Museum).

(8) Half-tone reproduction of J. Brown's engraving [see (1)] in *Religio Medici*, etc., publ. by Blackie and Son, London, 1906.

(9) Half-tone reproduction of same engraving in *English Literature*, by Edmund Gosse, publ. by Wm. Heinemann, London, 1903, Vol. III. p. 52.

(10) Photogravure in *Some Historical Portraits*, publ. by Oxford University Press, 1911.

C. *Reproductions from Bodleian Portrait.*

(1) Engraving by Hinchliff [said to be after Bodleian, but cloak certainly after White—see G (7)] in *Collected Works*, publ. by H. G. Bohn, London, 1852.

(2) Photogravure in *Religio Medici*, etc., publ. by G. Moreton, Canterbury, 1894.

D. *Reproductions from Norwich Portrait.*

(1) Lithograph of a charcoal drawing from this picture, by Cotman (Miles Edmund?), publ. by Ch. Muskett, Norwich (copy in Norwich Castle Museum).

(2) Wood-cut (poor) by J. Cooper, in *Varia: Readings from Rare Books*, publ. by Sampson, Low, Son and Marston, London, 1866.

(3) Same wood-cut as (2) (cut down, and *very* poor), in *Religio Medici*, etc., publ. by Sampson, Low, Son and Marston, London, 1869.

(4) Same as (2) in a re-issue of same edition with cancel title-page by Charles Scribner's Sons, New York, 1869.

(5) Same as (2) in a reprint of same book by Sampson, Low, Son and Marston, 1877.

(6) Collotype reproduction in *Souvenir of Sir Thomas Browne*, by Charles Williams, publ. by Jarrold and Sons, Norwich, 1905.

(7) Photogravure by Emery Walker in *An Alabama Student and other Biographical Essays*, by Sir Wm. Osler, publ. by Clarendon Press, Oxford, 1908.

E. *Reproductions from Van Hove's Engraving* (6th edn. *Pseudodoxia*, 1672).

(1) Photogravure in the *Collected Works*, publ. by E. Grant Richards, London, 1904–1907.

(2) Half-tone in *Religio Medici*, publ. by Cassell and Co., London, 1905.

(3) Photogravure in *Religio Medici*, etc., publ. by E. Grant Richards, London, 1906.

F. *Reproduction from Van der Banck's Engraving.* (*Certain Miscellany Tracts*, 1683, 1684.)

Engraving by W. C. Edwards in *Lives of British Physicians*, publ. by John Murray, London, 1830.

G. *Reproductions taken directly or indirectly from R. White's Engraving.* (*Collected Works*, 1686.)

(1) Engraving (no name) in *Alle de Werken van Thomas Brown*, publ. by de Widuwe van Steven Swart, Amsterdam, 1688.

(2) Engraving by Van der Gucht* in the *Posthumous Works*, publ. by E. Curll, London, 1712.

(3) Same as (2) in a reprint of the *Posthumous Works*, publ. by E. Curll, London, 1736.

* Blomefield says (in 1741) that Van der Gucht copied from the Norwich portrait, but he is mistaken (*Hist. of County of Norfolk*, Vol. III. p. 193).

(4) Engraving by Trotter, publ. separately by W. Richardson, London, 1798.

(5) Same engraving by Trotter in *Lives of Topographers and Antiquaries*, by J. P. Malcolm, publ. by R. Bickerstaff, London, 1815.

(6) Engraving by W. C. Edwards in *Sir Thomas Browne's Works*, publ. by Wm Pickering, London, 1836.

(7) Engraving by Hinchliff in *The Works of Sir Thomas Browne*, publ. by H. G. Bohn, 1852 (claims to be after Bodleian painting, but dress certainly copied from White).

(8) Engraving (unsigned), in *Religio Medici*, etc., publ. by Ticknor and Fields, Boston, 1862.

(9) Same as (8) in reprint of same, 1863.

(10) Same as (8) in reprint of same, 1868.

(11) Same as (8) in a re-issue of same with cancel title-page by Jas. R. Osgood, Boston, in 1872.

(12) Same as (8) in a re-issue of same with cancel title-page by Roberts Bros., Boston, in 1878.

(13) "Photomezzotype" reproduction of original engraving by White; in *Religio Medici*, etc., publ. by David Stott, London, 1892.

(14) Photogravure by Walker and Boutal of engraving by Van der Gucht [see (2)] in *Hydriotaphia, Urn Burial; with an Account of Urns found at Brampton*, publ. by Ch. Whittingham and Co., London, 1893.

(15) Engraving (no name) in *Religio Medici*, etc., publ. by J. M. Dent and Co., London, 1896 (not carried so far down as in White).

(16) "Photomezzotype" reproduction [same as (13)] in *Religio Medici*, etc., publ. by Smith, Elder and Co., London, 1898.

(17) Photogravure of engraving by Van der Gucht [see (2)] in *Religio Medici*, etc., publ. by Geo. Bell and Sons, London, 1898.

(18) Photogravure of original engraving by White, in *Religio Medici and Other Essays*, publ. by Gray and Bird, London, 1902.

(19) "Photomezzotype" reproduction [same as (13)] in *Religio Medici*, etc., publ. by Sherratt and Hughes, Manchester, 1902 (a re-issue, with cancel-title, of Smith, Elder and Co.'s edition) [see (16)].

(20) Same as (17) in a re-issue of same with cancel title-page, by Scott Thaw Co., New York, 1903.

(21) Half-tone reproduction of original engraving by White, in *Simples from Sir Thomas Browne's Garden*, publ. by B. H. Blackwell, Oxford, 1905.

(22) Half-tone reproduction of original engraving by White, in *Quaint Sayings from Works of Sir Thomas Browne*, publ. by Elliot Stock, London, 1905.

(23) Unsigned and very poor pen-and-ink drawing, apparently from White, printed by line-block, publ. by R. M. Gibbings and Co., London, 1905.

(24) Half-tone reproduction of original engraving by White, in *Golden Thoughts from Sir Thomas Browne*, publ. by John Lane, London, 1908.

(25) Photogravure of engraving by W. C. Edwards [see (6)] in *Religio Medici*, etc., publ. by Chapman and Hall, London, 1911.

In addition to the above, Simon Wilkin (in *Sir Thomas Browne's Works*, publ. 1835-6, Vol. II. p. 168) alludes to a mezzotint portrait of which he has seen a copy in an extra-illustrated Blomefield's *Norwich*, in the Dean and Chapter's Library at Norwich (an unsuccessful attempt was made to find this portrait; a search under more favourable circumstances might reveal it).

APPENDIX III.

It occurred to the Editor that a search in the records of Winchester College might throw some light on Sir Thomas Browne's history. He made an application to Mr Herbert Chitty, the Bursar, who not only most kindly forwarded the information given below, but also a list of the editions of Browne's works in the various College libraries, which has been communicated to Dr Keynes. The material is of great interest.

Register of Scholars admitted to the College. Vol. I. p. 111.

"Nomina Scholarium admissorum a 19° Augusti 1616 viz. a fine Electionis tunc celebratæ usque ad promixam [*sic*] Electionem anni sequentis 1617."

[2nd name :—]

"Thomas Browne de parochia sancti Michaelis in le Cheape comitatu Midlesex 10 annorum ad festum sancti Michaelis præteritum admissus 20° Augusti 1616—London. [diocesis]." [Marginal note :—] "recessit sponte. Medicus Dr. Norvici."

Muniment Room.

"*Liber Prothocollorum de iuramentis tam Sociorum quam Scholarium de novo compositus A° Dⁿⁱ* 1576."

"Nomina Scholarium juratorum ultimo die Septemb. Anno Dni 1620."

[Amongst other names :—]

"Thomas Browne de St Foster comitatu Middlesex, Londō dioc."

The notary who made the record was "Johannes Harmar civilista London. Diocesis publicus authoritate Regia notarius."

Under Rubric V of the College Statutes the scholars had to take an oath to obey the Statutes, etc., "post quintumdecimum annum completum."

Muniment Room.

Election Roll 1622. Roll of Scholars selected for New College, Oxford.

"Oxon. Anno Domini 1622 a decimo die Augusti ad decimum septimum eiusdem mensis."

[The 11th boy on this Roll is :—]

"Thomas Browne de parochia Scti Michaelis in Le Chepe de comitatu Midlesex annorum 17 festo Michaelis præterito—London. [diocesis]."

[He was too low down on the Roll to succeed to a vacancy at New College.] The Roll is signed by "Robertus Pinck, Custos" [Novi Collegii], "Gullielmus Oldys" and "Thomas Boothe," "Examinatores": "Nicholas Love, Custos [Win. Coll.], Henricus Madgniake, Vice-custos, Hugo Robinson, Informator."

[Thus examiners through the ages!]

Fellows' Library.

Warden Nicholas's MS. Book relating to the building of "the new School at Winton Colledge" begun to be built in Sept. 1683 and finished June xi, 1687.

"Nomina Nobilium et Dignissimorum Virorum, Qui surgenti Wichamicæ Scholæ Magnificentiæ auxiliares manus admoverunt Quantumque Collegium Wintoniense gratia valuit spontaneis Beneficiis plurimisque docuerunt."

[Inter alios :—]

"Thomas Brown Eques. Med. Dr. Quondam hujus Coll. Comēnsalis, Dedit . . 10 0 0".

ADDENDA

Concerning the "Zoust Shakespeare."

Since the above memoir was printed off we have heard from Mr Cecil Lister Kaye, who very kindly gives us some information with regard to the portrait said to be of Shakespeare which was bought from William Douglas by his great-grandfather, Sir John Lister Kaye, in 1824*. This portrait is still in the possession of the family, at Denby Grange, Wakefield and papers relating to it are also preserved there. Mr Cecil Lister Kaye informs us that these state that the picture came into the possession of Wm. Douglas on his marriage with a daughter of General Otley, and that the last-named had owned it for sixty years. Also that Mr Douglas maintained that the picture was painted by Cornelius Janssen (1593—1664?).

Of Janssen we know that he was practising as a portrait-painter in London in 1618, and for the next two decades was much in demand among the nobility and gentry. In 1636 he moved to Canterbury where he continued his work until he left England for Holland in 1643†.

Shakespeare died in 1616 at the age of fifty-two, Janssen being twenty-three years old at the time. It is very improbable that the young Janssen would have been called upon to execute his portrait, and it is quite impossible for him to have done so when Shakespeare was as youthful as the Wellcome version of this picture (Plate IX) represents him to be. Janssen might however have painted Sir Thomas Browne, but the portrait described in the next paragraph gives no countenance to the 'Zoust' picture being that of a younger Browne.

Discovery of a new Portrait, the Le Strange, of Sir Thomas Browne (Frontispiece).

Another and most important portrait of Sir Thomas Browne has come to light since this monograph was printed off. It is now in the possession of Mr Francis Wellesley, J.P., of Westfield Common, near Woking, who lately bought it from a dealer in Norwich; he, again, had purchased it at a recent sale of pictures, etc., the property of the late Mr Hammond Le Strange, of Hunstanton. The identity of the figures portrayed, a man and a woman, was unknown. It was shewn to the Director of the National Portrait Gallery, who immediately recognized in the male portrait the features of Sir Thomas Browne. The young woman by his side is doubtless Dame Dorothy, and from the apparent ages of the two we should say it was painted somewhere about the date of their marriage (1641), he being thirty-six and she nineteen at the time. The picture ($9\frac{1}{8}'' \times 7\frac{1}{4}''$) is painted upon an oak panel‡, and is contained in a gilt oak frame of a mid-17th century pattern, that known as the 'Mask,' or Sunderland pattern. The *Le Strange*, or as formerly spelt, *L'Estrange* family has had its seat at Hunstanton since Norman times. An ancestor of the late Mr Hammond Le Strange, Sir Hamon L'Estrange (1583—1654), was a personal friend and correspondent of Sir Thomas Browne. A letter from him to Sir Thomas has been handed down to us§, and the Harleian MSS. include an 80-page manuscript of *Observations on the Pseudodoxia* by him. Probably his sons also, contemporaries of Sir Thomas in age, like him men of letters, and like him Royalist in sympathy, were reckoned among Sir Thomas Browne's friends. In any case it seems most probable that the possession by the L'Estranges of the portraits of Sir Thomas and Lady Browne is accounted for by the friendship between the two families, and the evidence contained in the picture and frame points to this being a contemporary and original portrait; moreover we find at last, in this picture, a head which answers in every detail to what the skull would indicate as the appearance of the living head. The nose long and slightly aquiline, the cheek-bone projecting, the forehead normally broad at the level of the eyebrows, but retreating

* See our pp. 21—25. † *Dict. Nat. Biog.*

‡ At present it is streaky, with bad varnishing, as is seen in our frontispiece reproduction.

§ *Sir Thomas Browne's Works*, ed. by Simon Wilkin, 1836, Vol. I, p. 369.

and narrowing abruptly; and, above all, exhibiting a lowness that no other portrait examined thus far has matched. Here we have the forehead that we knew a faithful portrait must shew, and the back of the head has a capacious appearance which is also confirmed by the skull. That this portrait is intimately connected with many, if not all, of the other pictures, is easily seen. The two streaks of hair on the forehead are there, almost exactly as in the engravings by Van Hove and White, and in the Royal College of Physicians' painting. The outline of the hair is the familiar outline, and the little silhouetted curl so often referred to can be seen clearly in the painting itself, though it is almost lost in the reproduction. In the painting we can also detect the buttons fastening the inner coat, just as in White's engraving and some others. The faint lines of the coat and cloak resemble those indicated in the Norwich picture. The collar is of the Norwich and Gunton pattern.

Space does not permit us to discuss in detail the relation of this picture to the others. We can only suggest briefly what seem to us the conclusions that must be drawn from a comparison between them. Considering the three portraits which we now have with the earlier type of collar, the Le Strange, the Norwich and the Gunton, we are disposed to transfer to the Le Strange the honour, if it be one, of having served as original to the Gunton copy. Whether the Norwich is also a copy from the Le Strange, or a portrait painted independently at about the same period, is a moot point; much could be said in favour of either view.

Again, as regards the Buccleuch miniature: if this be a copy of another picture, we should say it was the Le Strange rather than the Norwich portrait which inspired it. As to the engravings and the Royal College of Physicians' painting, we cannot yet displace the miniature with its square-cut collar and turned-up corner, from the position assigned to it in regard to these. But from the close resemblance in the matter of the stray locks of hair on the forehead, which exists between the Le Strange and these others, we must suppose that both the Le Strange and the miniature were accessible to the later artists. Or else it may be that a copy of the Le Strange, but with square-cut collar, is either somewhere or was once in existence, and served as original both to the miniature and to these other portraits. Perhaps some other country-house in Norfolk still holds the picture which will supply the link. But whether this be so or not, we feel at least satisfied that we have in the Le Strange portrait one that survives triumphantly the test of the skull, and may be counted a true portrayal of the living face of the author of the *Religio Medici*.

We have to thank Mr Wellesley for the ready permission he has given for the reproduction of this remarkable addition to the Browne portraits. He has increased greatly the value of the present study, and our readers, we feel sure, will pardon the delay the preparation of the frontispiece has involved in the issue of this monograph. It seemed impossible to further postpone the issue until the varnish was removed from the picture, which Mr Wellesley most kindly offered to have done within a month.

Fig. 2. Miniature of Sir Thomas Browne in the Buccleuch Collection. Recently on loan to the Victoria and Albert Museum. (By permission.)

Fig. 1. Plumbago Drawing of Sir Thomas Browne in the National Portrait Gallery, attributed to Loggan. (By permission.)

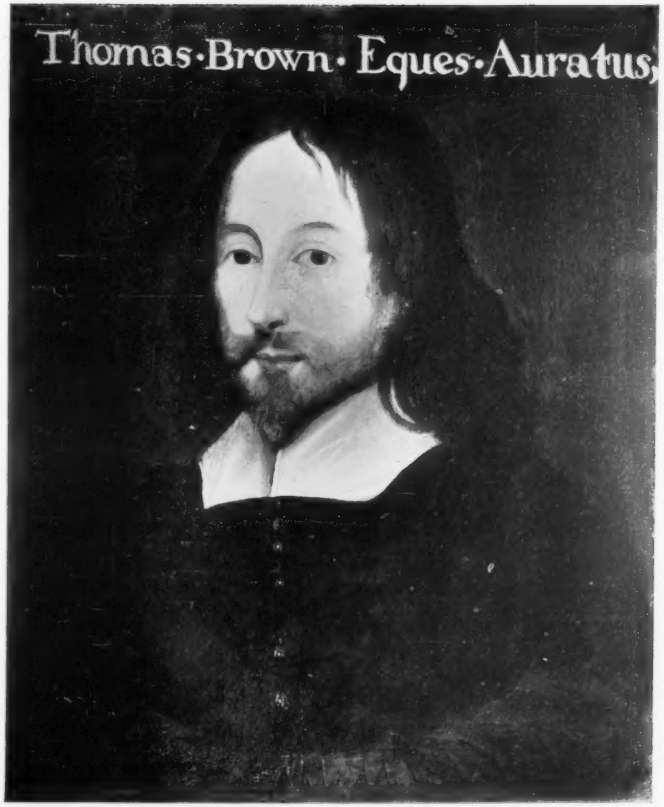


Fig. 3. Painting of Sir Thomas Browne at the Royal College of Physicians. (By permission.)

Fig. 4. Painting of Sir Thomas Browne at the Bodleian Museum, Oxford. (By permission.)

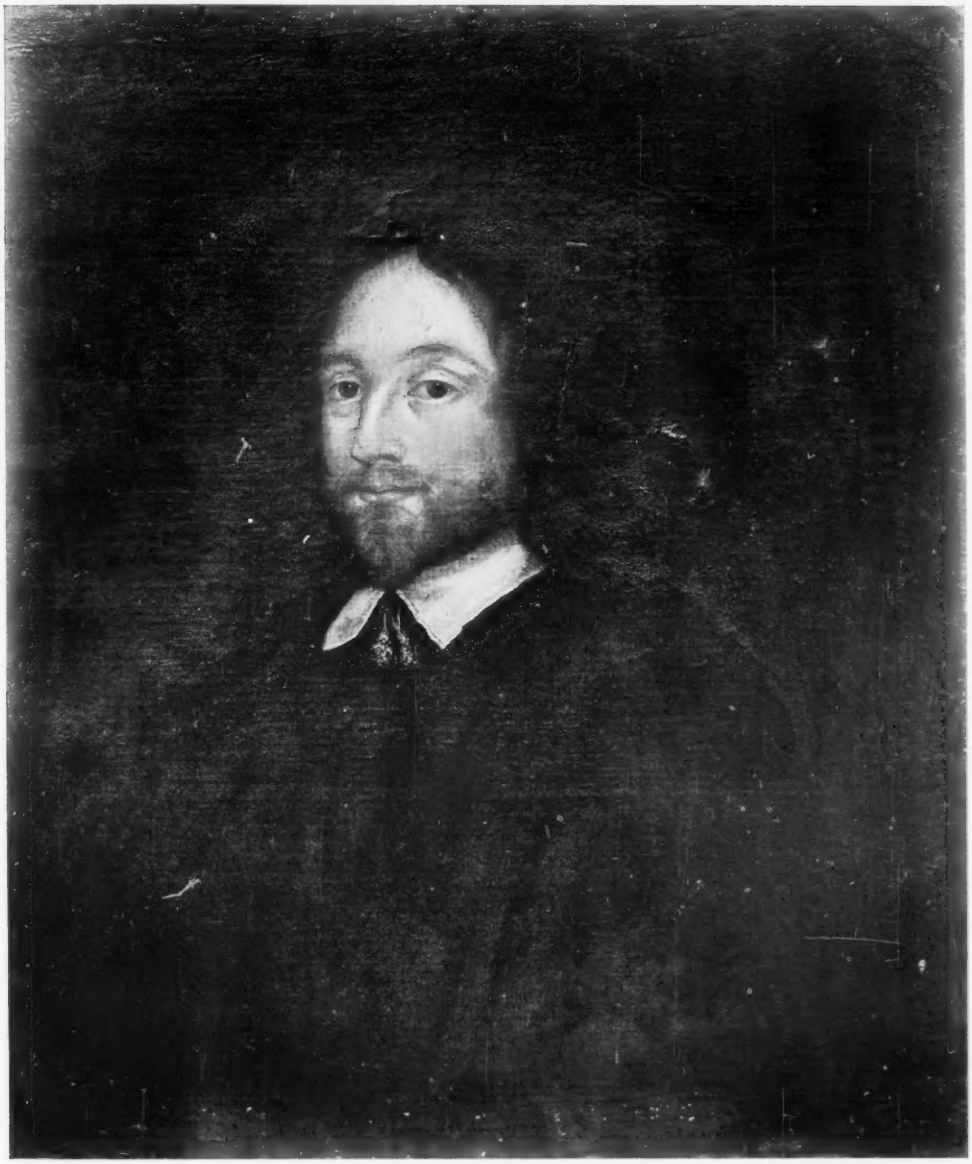


Fig. 5. Painting of Sir Thomas Browne in the Vestry, St Peter Mancroft, Norwich. (By permission.)

Fig. 6. Painting of Sir Thomas Browne at Gunton Hall, Norfolk. (By permission.)

Fig. 7. The alleged Portrait of Sir Thomas Browne at Sherborne House. (By permission.)

Fig. 8. The "Dobson" Group in the possession of the Duke of Devonshire. (By permission.)

Fig. 8 (bis). The "Dobson" Group from a photograph in the possession of the Norwich Castle Museum. (By permission.) The original of this photograph as will be seen on comparison with Plate VI must differ very considerably from the Duke of Devonshire's picture.

Fig. 9. Engraving issued as a portrait of Sir Thomas Browne with the 5th Edition of the *Pseudodoxia*, 1669.

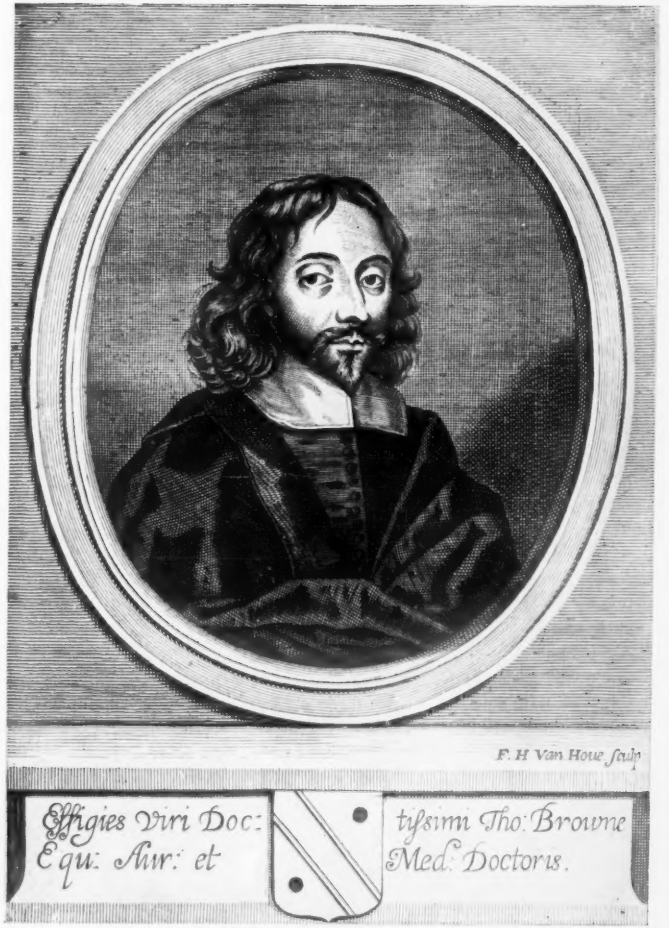


Fig. 10. Van Hove's engraving of Sir Thomas Browne issued with the 6th Edition of the *Pseudodoxia*, 1672.

Fig. 11. Van der Banck's engraving of Sir Thomas Browne issued with the *Certain Miscellany Tracts* in 1683 and 1684.

Fig. 12. Robert White's engraved portrait of Sir Thomas Browne issued with the *Collected Works*, 1686.

Fig. 13. A Painting said to be of Sir Thomas Browne in the Wellcome Historical Medical Museum. (By permission.)

Fig. 14. The Simon mezzotint of the so-called Soest portrait of Shakespeare. To indicate that the Wellcome portrait of Sir Thomas Browne is what passes as the Soest Shakespeare.

Fig. 15. Drawing made mid-nineteenth century by Miles Edmund Cotman of the Painting of Sir Thomas Browne in the Vestry of St Peter Mancroft, Norwich. To indicate the supposed form of the hair at that time.

Fig. 16. Portrait of Dr Edward Browne, son of Sir Thomas Browne. To indicate that the receding forehead of the latter appears to have been a characteristic of the former.

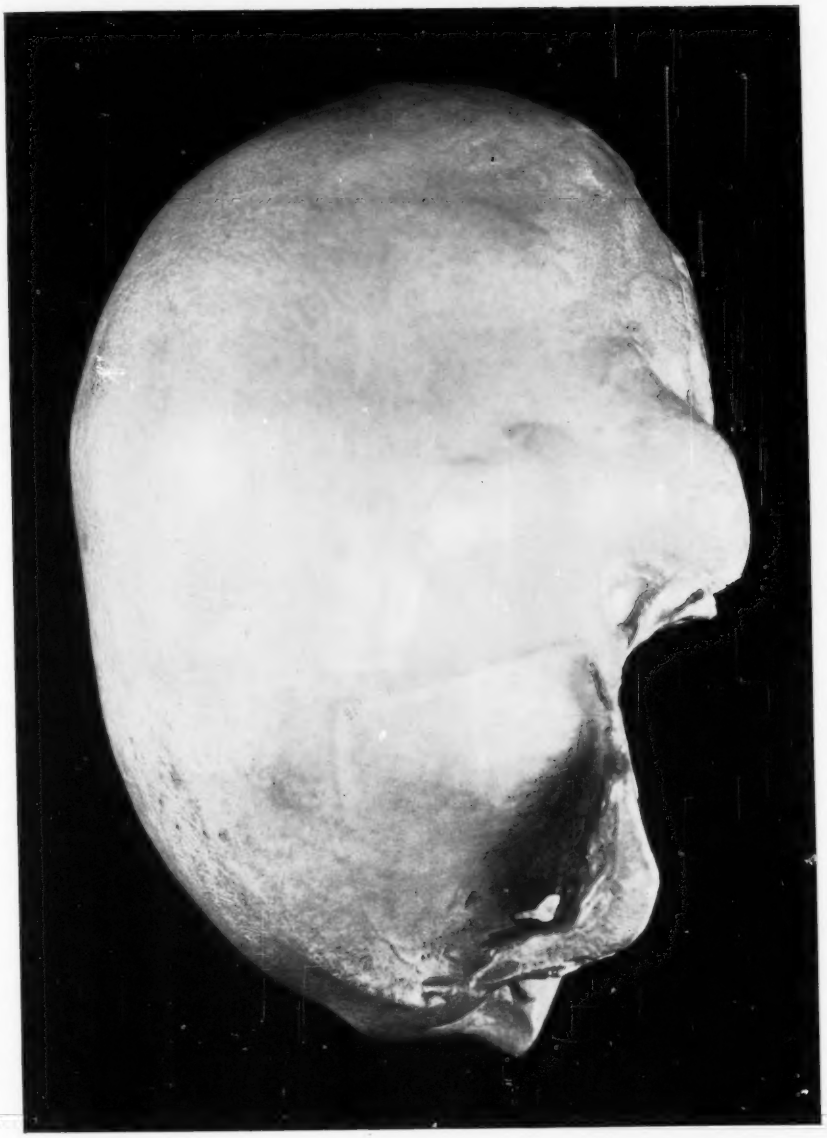
A. Photograph of skull recently re-interred.

B. Photograph of cast of 1840–1 in the Norwich Castle Museum.

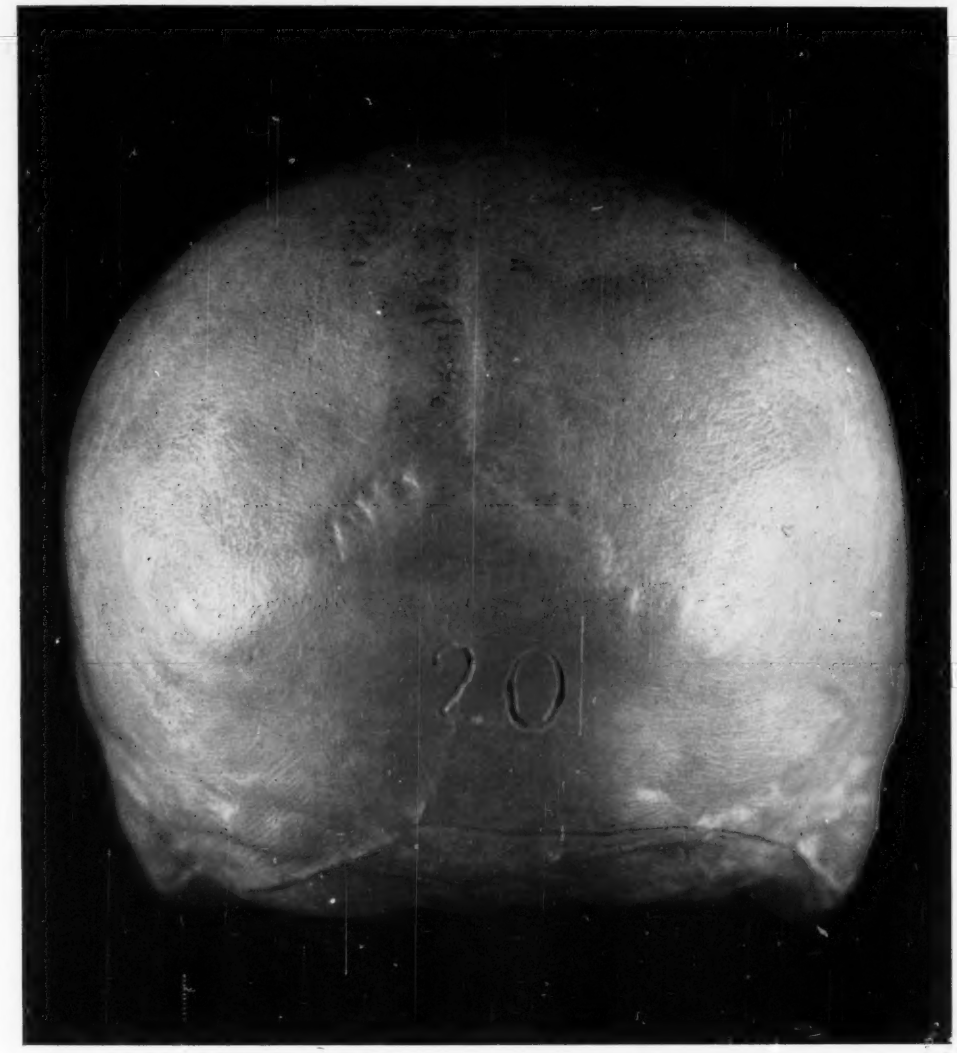
Fig. 17. Comparison between the lambdoid suture of the skull re-interred in 1922 as Sir Thomas Browne's and the same suture of the cast presented in 1841 to the Norwich Castle Museum as that of his skull. The coffin is known to have been opened in 1840.

M. S.
Hic situs est
THOMAS BROWNE M.D. et Miles
Aº 1605 LONDINI natus
Generosa Familia apud UPTON
In agro CESTRIENSI oriundus
Schola primum WINTONIENSI postea
In Coll. PEMBR.
Apud Oxonienses bonis literis
Haud leviter imbutus.
In urbe hac NORDOVICENSI medicinam
Arte egregia & felici successu professus.
Scriptis quibus tindi. RELIGIO MEDICI.
Et PSEUDODOXIA EPIDEMICA aliisque
Per Orbem notissimus.
Vir Prudentissimus, Integerrimus, Doctissimus,
Obiit Octobr. 19. 1682.
Pie posuit moestissima Conjux
Dª Dorothy Br.
Near the foot of this Pillar lies Sr Thomas Browne Kt and
Doctor in Physick, Author of Religio Medici, and other
Learned Books, who practic'd Physick in this City 46 years,
died Octo 19. 1682, in the 77th year of his Age. In Memory of
whom, Dame Dorothy Browne, who had been his affectionate
Wife 41 years, caused this Monument to be erected.

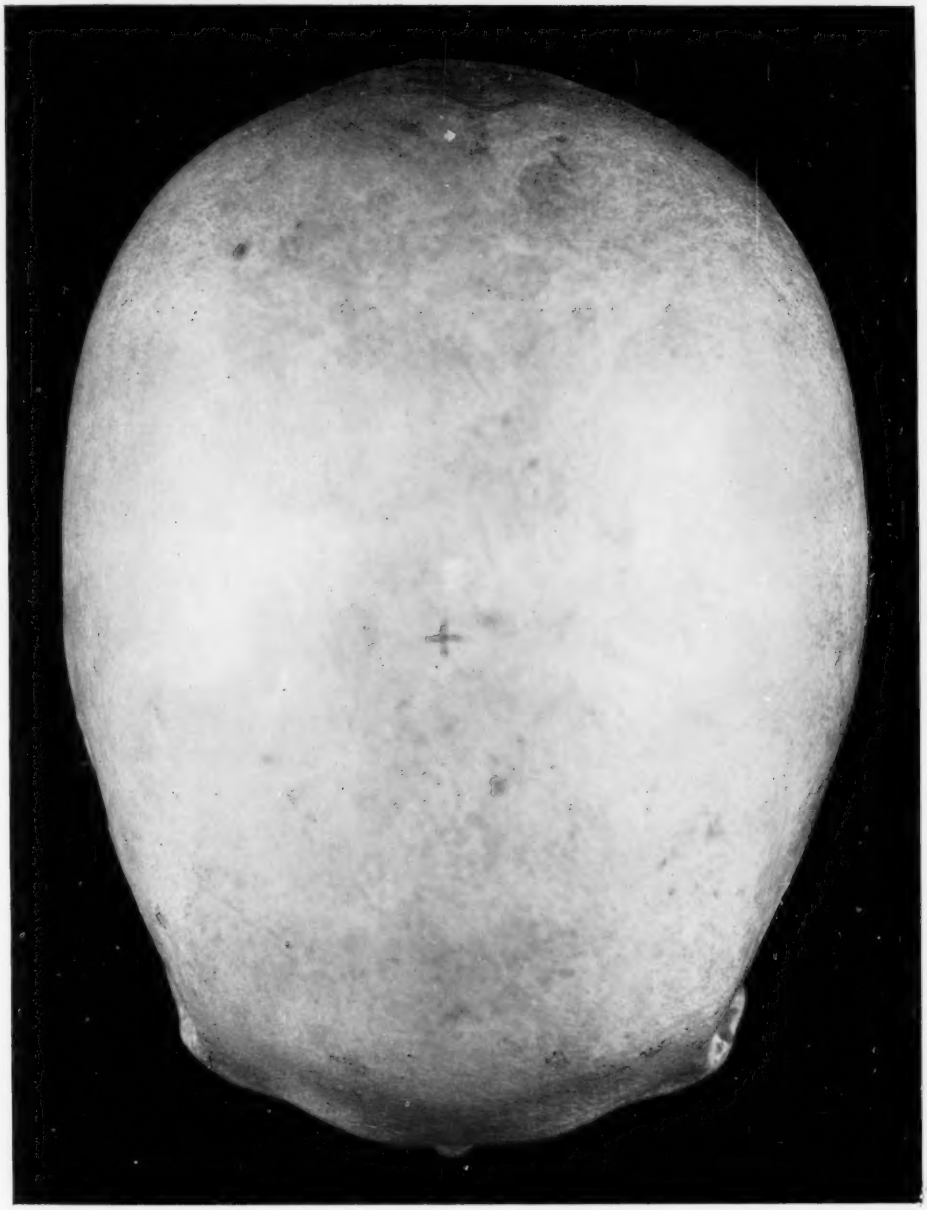
The Memorial Tablet to Sir Thomas Browne in St Peter Mancroft, Norwich. Reproduced from the engraving in Vol. III of Wilkin's edition of the *Collected Works*, 1838.



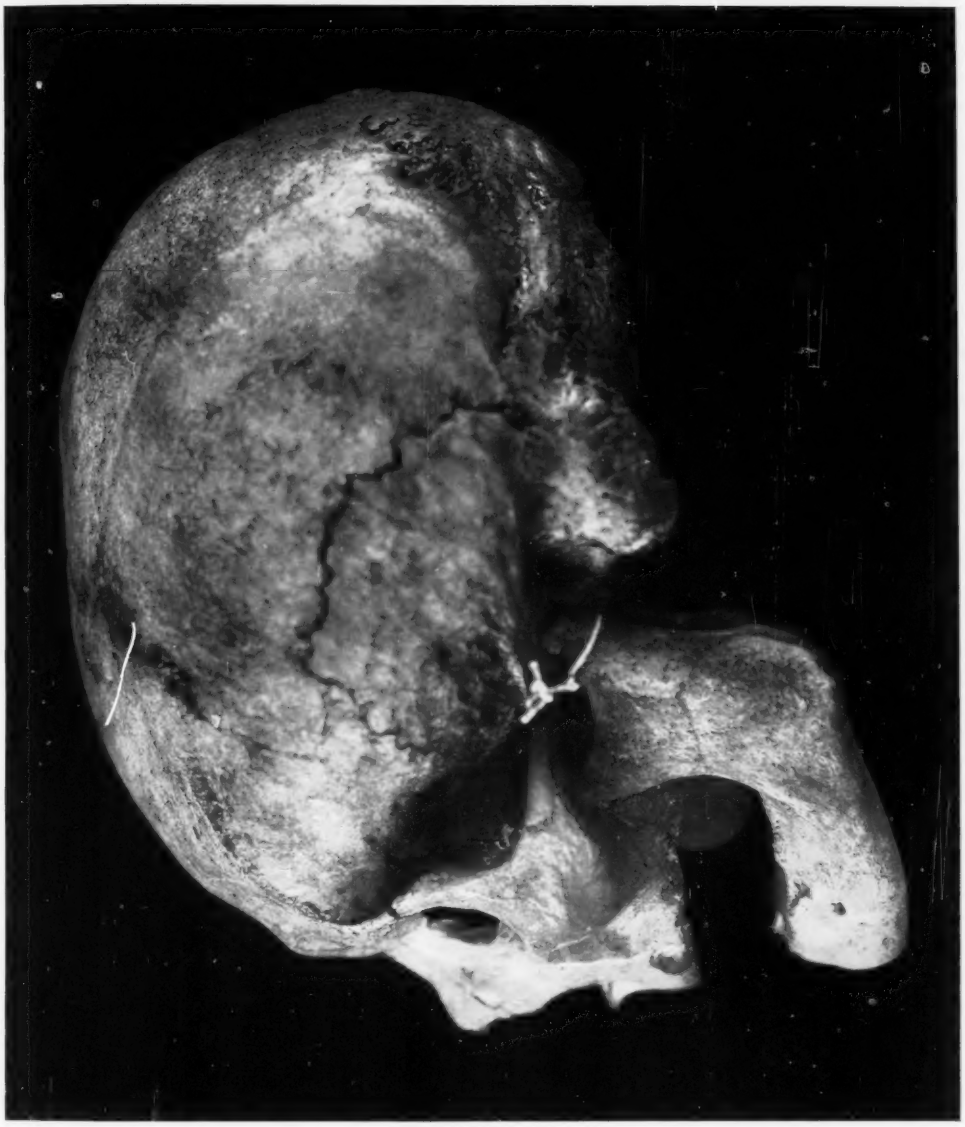
Cast of Skull taken in 1840–1. *Norma lateralis* (left profile).



Cast of Skull taken in 1840–1. *Norma occipitalis.*



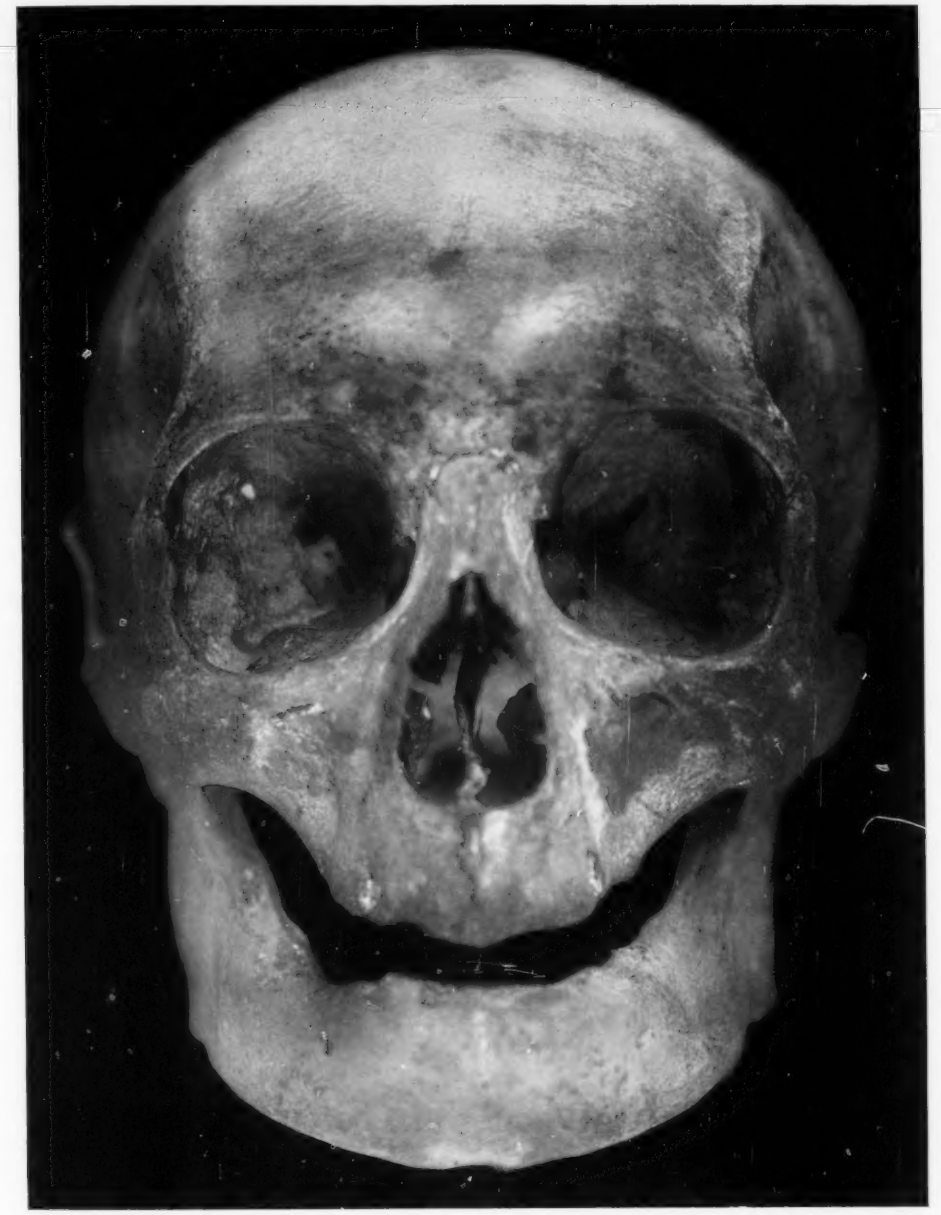
Cast of Skull taken in 1840–1. *Norma verticalis.*



Sir Thomas Browne's Skull. *Norma lateralis* (left profile).

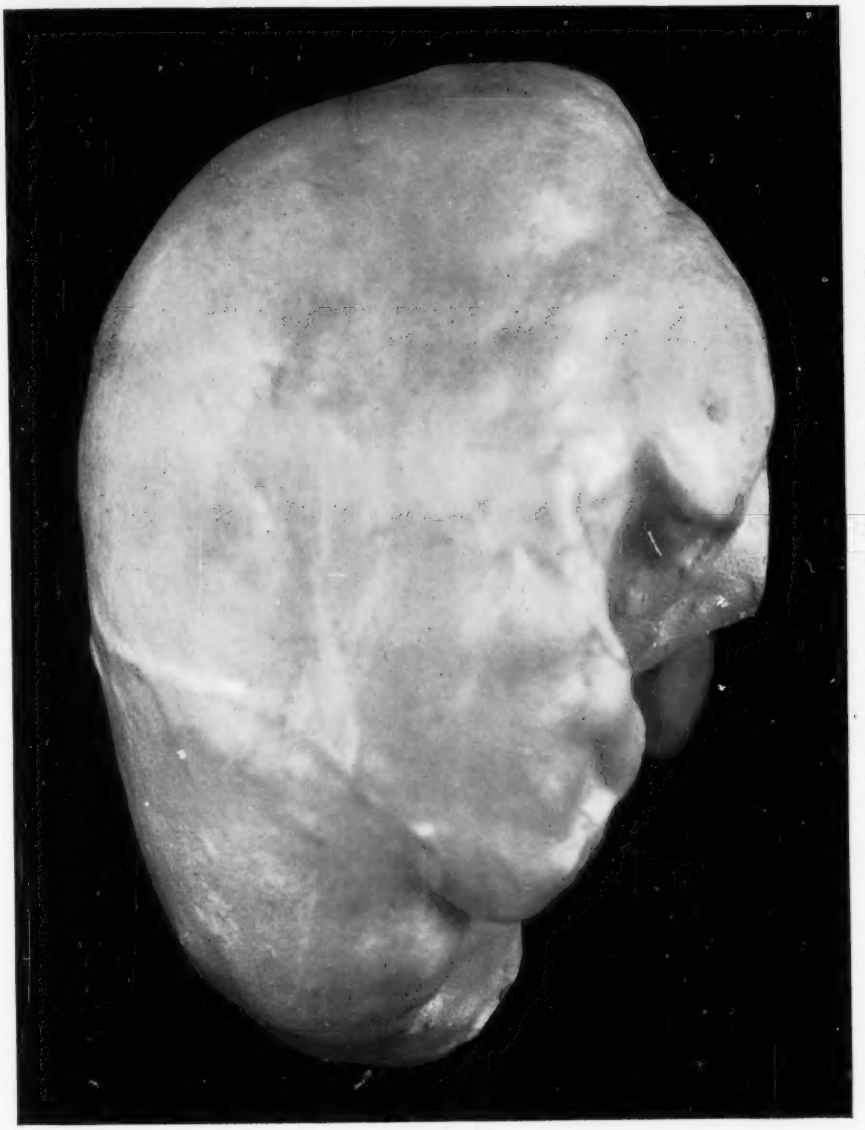
Sir Thomas Browne's Skull. *Norma occipitalis.*

Sir Thomas Browne's Skull. *Norma verticalis.*



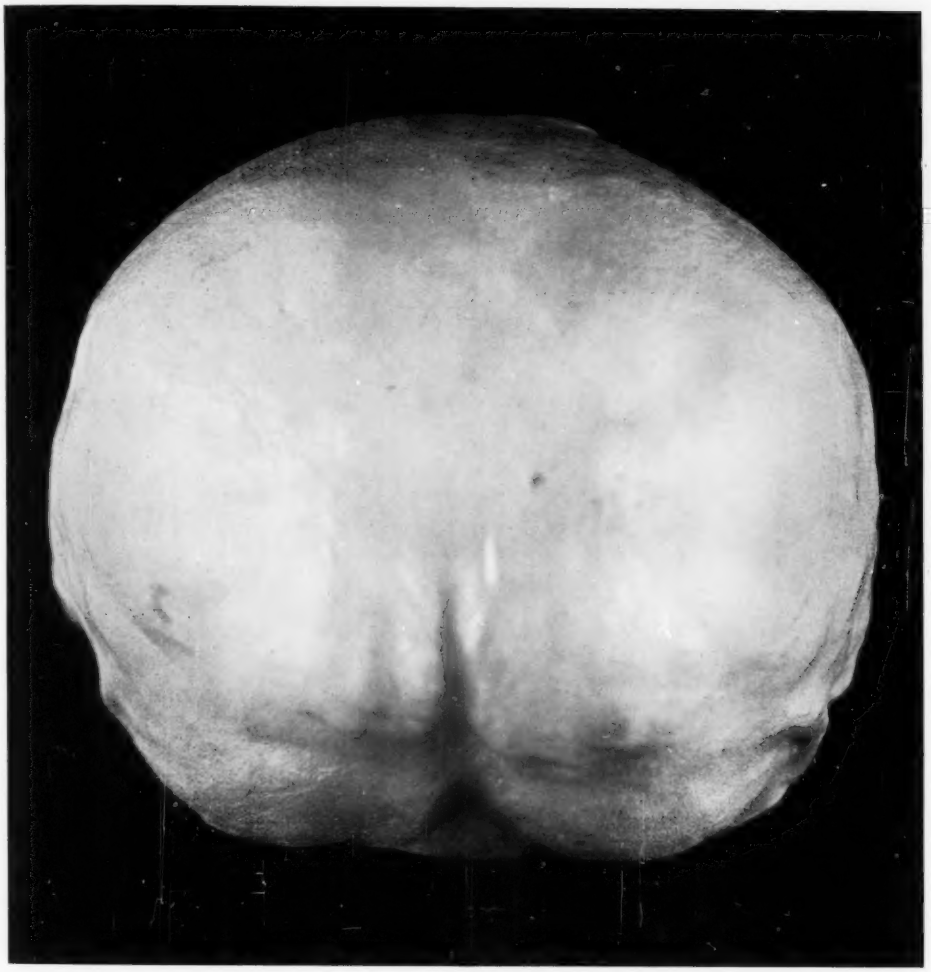
Sir Thomas Browne's Skull. *Norma facialis.*

Sir Thomas Browne's Skull. *Norma basalis.*



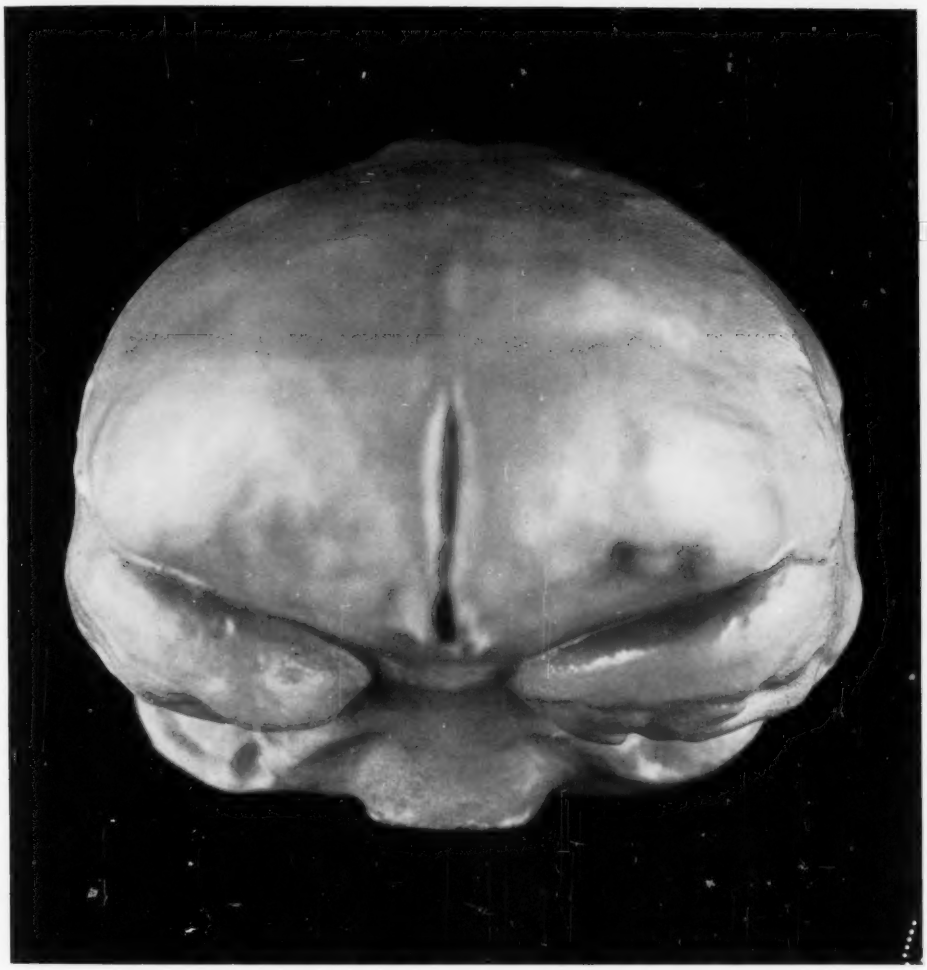
Endocranial Cast of Sir Thomas Browne's Skull. *Norma lateralis* (left profile).

Endocranial Cast of Sir Thomas Browne's Skull. *Norma lateralis* (right profile).

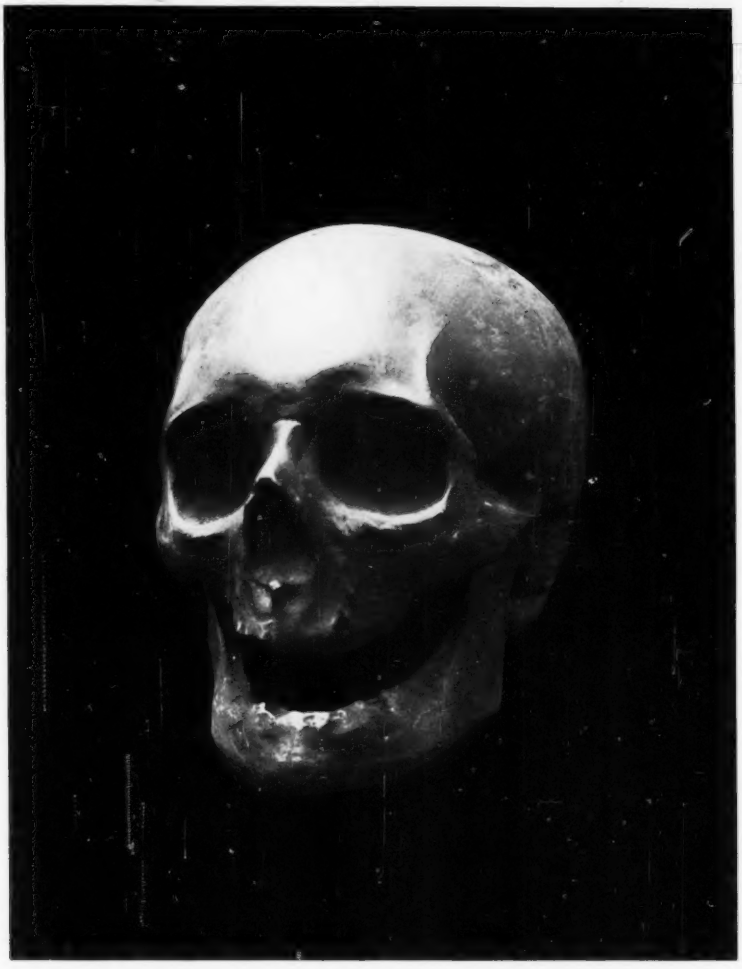


Endocranial Cast of Sir Thomas Browne's Skull. *Norma occipitalis.*

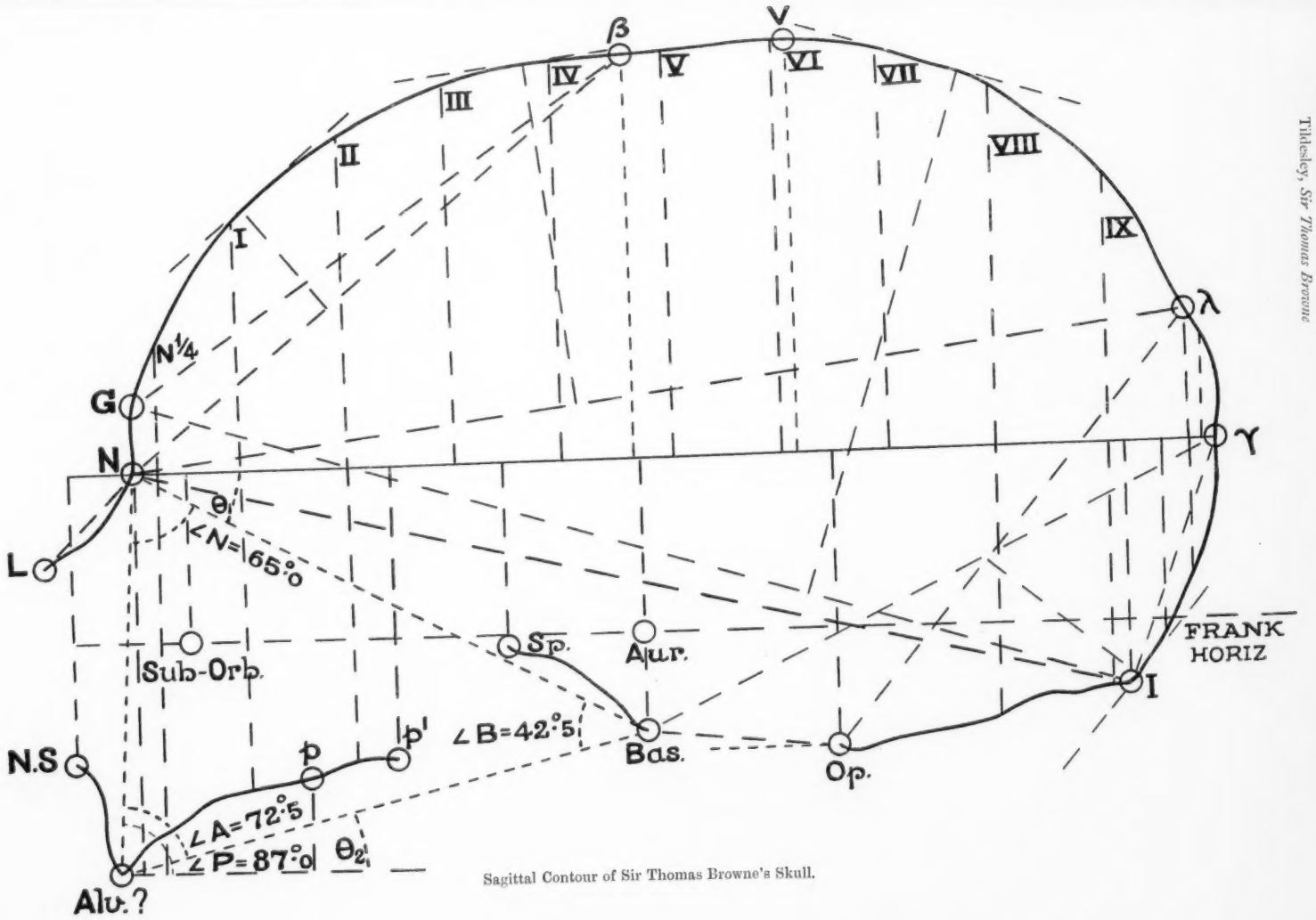
Endocranial Cast of Sir Thomas Browne's Skull. *Norma verticalis.*



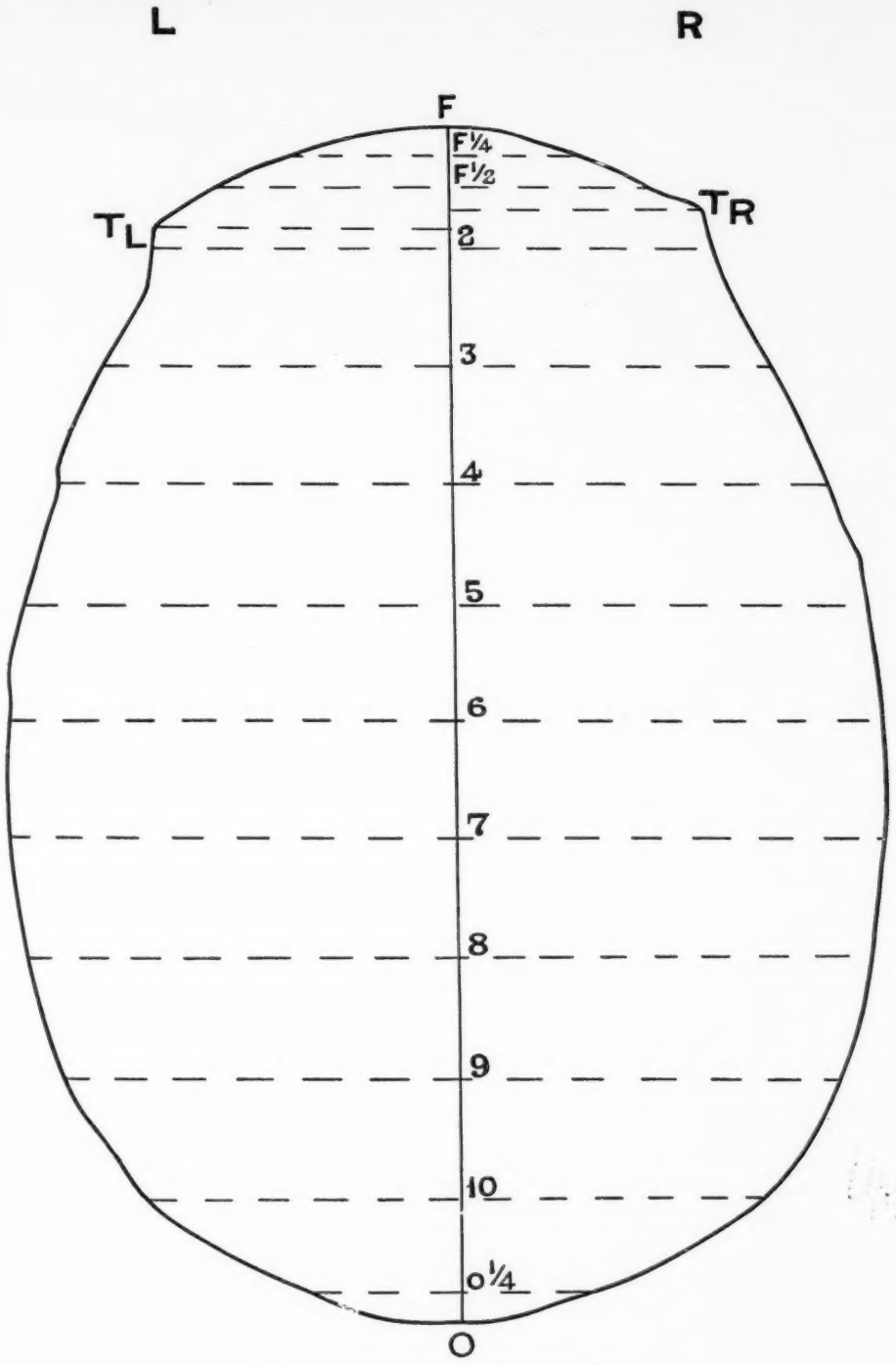
Endocranial Cast of Sir Thomas Browne's Skull. *Norma frontalis.*



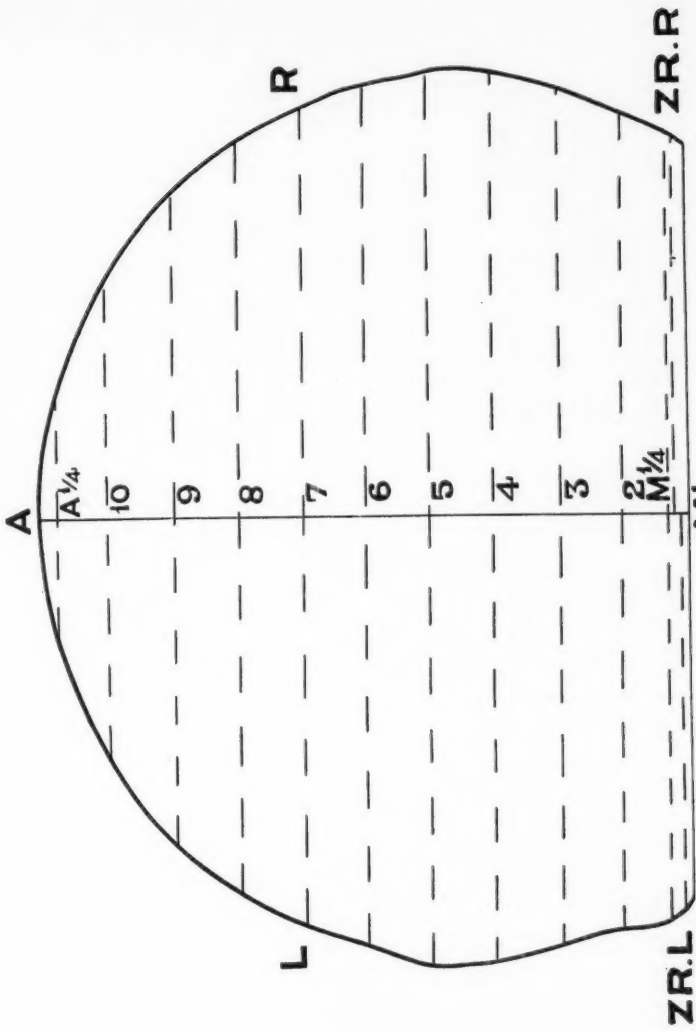
Cast of Sir Thomas Browne's Skull taken in 1922, and arranged three-quarter face to correspond in aspect with the Buccleuch miniature : see Plate I.



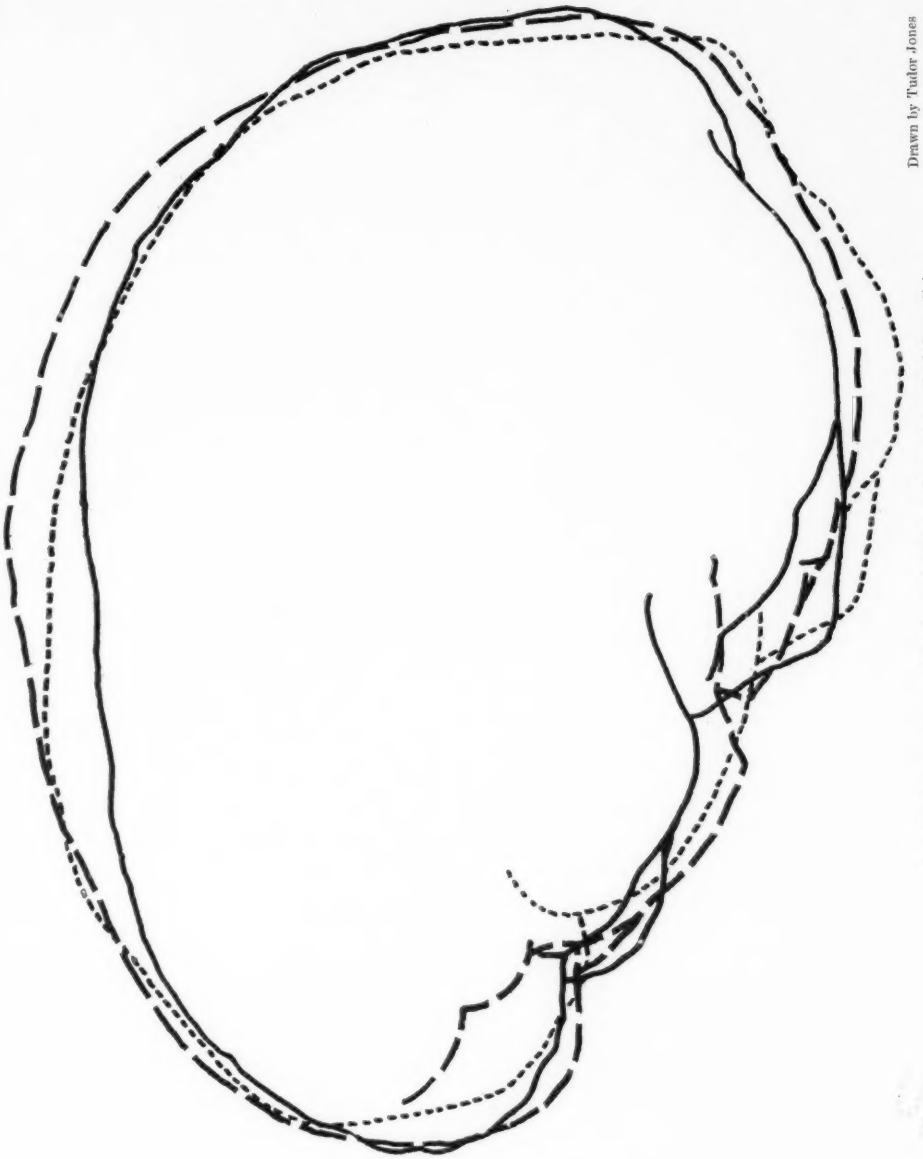
Sagittal Contour of Sir Thomas Browne's Skull.



Horizontal Contour of Sir Thomas Browne's Skull.



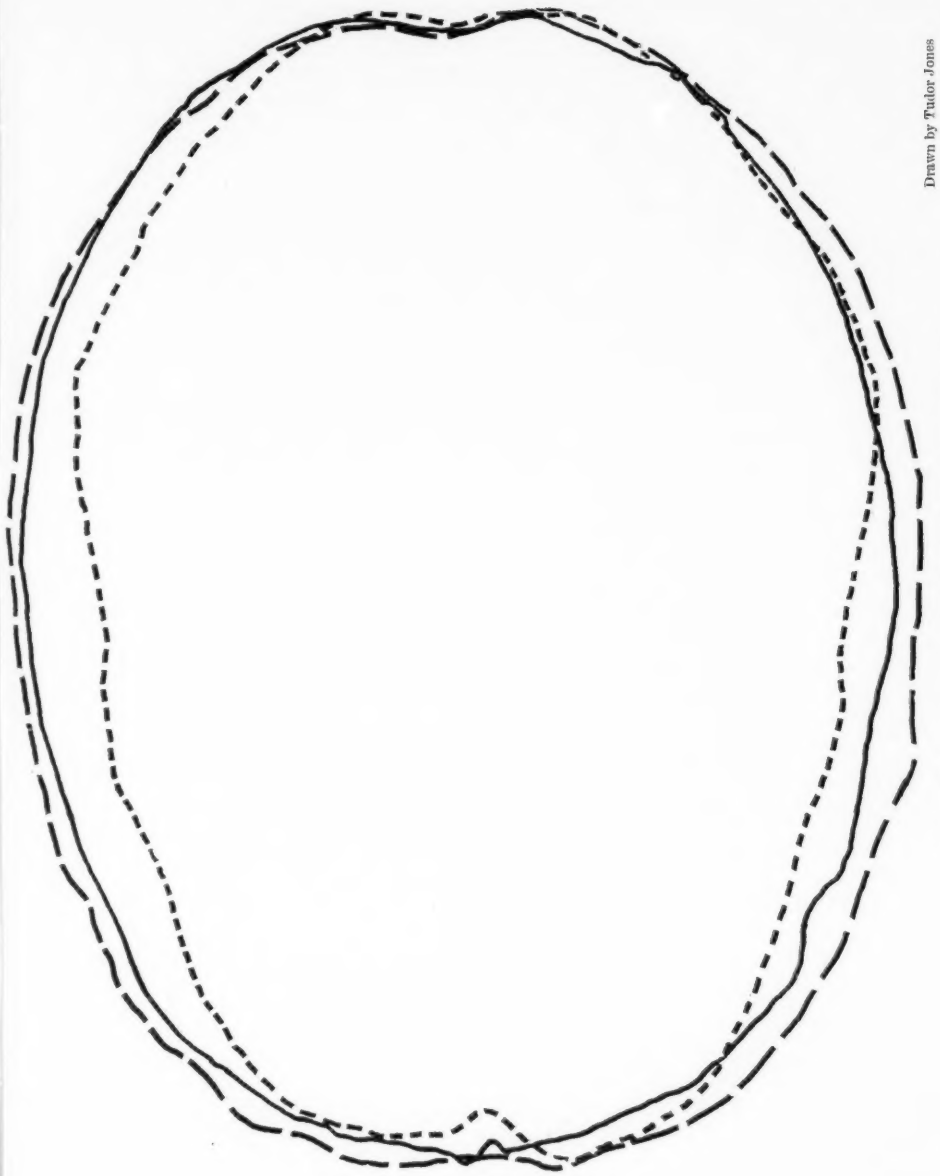
Transverse Contour of Sir Thomas Browne's Skull.



Drawn by Tudor Jones

Orthographic Projections of Endocranial Casts. *Norma lateralis* (left profile).

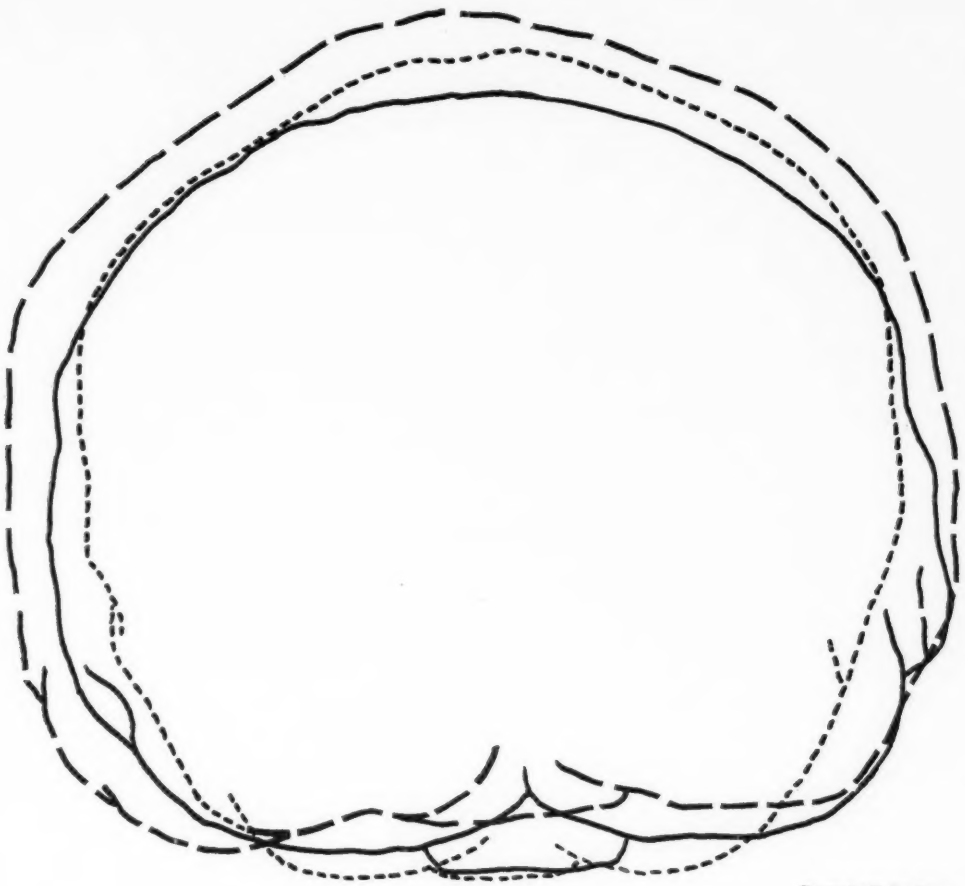
— — — — Dean Swift ——— Sir Thomas Browne - - - - - - English Male of average cranial capacity



Drawn by Tudor Jones

Orthographic Projections of Endocranial Casts. *Norma verticalis.*

— — — — Dean Swift ——— Sir Thomas Browne - - - - - - English Male of average cranial capacity



Drawn by Tudor Jones

Orthographic Projections of Endocranial Casts. *Norma occipitalis.*

— — — Dean Swift
———— Sir Thomas Browne
- - - - - - - - - English Male of average cranial capacity

ON THE GENERAL FORMS OF BIVARIATE FREQUENCY DISTRIBUTIONS WHICH ARE MATHEMATICALLY POSSIBLE WHEN REGRESSION AND VARIATION ARE SUBJECTED TO LIMITING CONDITIONS.

By Professor SEIMATSU NARUMI.

PART I (*to be continued* *).

(1) There have been a number of attempts to reach a general bivariate frequency surface when either variate is distributed with any degree of skewness. Most of these have proceeded from assumptions based on some probability hypothesis, as when we seek a surface closely reproducing the double hypergeometrical series. The present investigation starts on entirely different lines; it considers two essential factors of bivariate frequency distributions: (i) their lines of regression and (ii) the variation of the arrays of one variate for a constant value of the second variate. By line of regression we mean the curve formed by a series of corresponding points in the base line of the arrays of one variate for constant values of the second variate. These points may be the means of the arrays, or their modes or medians, or in fact any series of points defined in the same manner for each array. The locus of such points is the regression line. Let the regression line of the y variate on the x variate be $y=f_2(x)$, and of the x variate on the y variate be $x=f_1(y)$.

Now we may suppose the frequency curve of any array to be transferred to the corresponding point on the regression line as origin, and if z be the ordinate so that $z\,\delta x\,\delta y$ gives the frequency of individuals with characters between x and $x+\delta x$ and y and $y+\delta y$, then the equation to the y arrays will be of the form

$$z=\Psi_2\{y-f_2(x)\},$$

and this may be taken as the equation to the surface, if we remember that all constants in Ψ_2 may be looked upon as functions of x.

Now if all arrays of y for a given x have the same shape—but of course not the same size—then the variations of all arrays of y for a given x will be the same; and we define the arrays as *completely homoscedastic*. If these arrays be brought to a common origin as determined by the regression line they will be a system of similar and similarly placed curves defined by their origin as centre of similitude.

* The second part of this paper deals with further possible functional frequency surfaces, and the third part with the criteria for fitting and methods of fitting.

In this case it is needful for the surface to be of the form

$$z = \Phi_2(x)\,\Psi_2\{y - f_2(x)\}.$$

If the surface be completely homoscedastic for both variates, it must take the form

$$z = \Phi_1(y)\,\Psi_1\{x - f_1(y)\} = \Phi_2(x)\,\Psi_2\{y - f_2(x)\},$$

which is a functional equation defining bivariate frequency unlimited as to regression but doubly and completely homoscedastic.

It is possible, however, that the arrays are not completely homoscedastic; the most general case that we can consider here will be completely homoscedastic, if the units in which we measure variation in each array be modified. The reader will best appreciate this modification, if he thinks in terms of standard-deviations and supposes the standard-deviations to vary from array to array. If then the variate be measured in terms of its standard deviation, the arrays will give frequency curves which are similar, or they will now be completely homoscedastic. We need not however think in terms of standard-deviations; we may simply alter the scale of measurement of the variate in each array, the scale in the y-arrays varying with x and in the x-arrays varying with y. Let these scales of measurement, which reduce the array-curves to complete homoscedasticity, be defined by $1/F_2(x)$ and $1/F_1(y)$.

Then the most general functional equation to our surface of frequency will be

$$z = \Phi_1(y)\,\Psi_1[\{x - f_1(y)\}\,F_1(y)] = \Phi_2(x)\,\Psi_2[\{y - f_2(x)\}\,F_2(x)],$$

and the purpose of the present paper is to determine the form of this surface when $f_1(y)$, $f_2(x)$, $F_1(y)$, $F_2(x)$ are limited in certain ways. For example we may take $f_1(y)$ and $f_2(x)$ to be linear functions, i.e. we may impose linear regression on both variates. Or, we may make $F_1(y)$ and $F_2(x)$ constant, i.e. impose complete homoscedasticity on both variates. Or, we may make regression and scedasticity different for the two variates, and inquire under such limiting conditions what frequency surfaces are mathematically possible. As we shall see the possible mathematical surfaces are very narrowly limited. We cannot combine arbitrary regression lines with quite arbitrary array variation. If we know the nature of the regression and of the array variation we can predict what type of frequency surface is alone possible, without any appeal to the general laws of probability. In this way observation of regression and of the scedasticity of arrays leads us directly to the suitable frequency surface. The reader who prefers it can think in terms of the regression line of *means* and of a scale determined by the *standard-deviation* of an array. But this is really to narrow somewhat the problem. There is much physically to support the conception that all the arrays of one variate, if reduced to their proper origin and proper scales, will be similar curves, i.e. follow the same law of partial variation. And in the present state of statistical science this appears a wide enough hypothesis to start our inquiry as to the possible nature of bivariate frequency surfaces.

(2) We now proceed to the consideration of various special cases.

(i) Let us suppose $F_1(y)$ and $F_2(x)$ constants, or the cross sections similar

curves without any reduction to a variable scale. Then our fundamental equation to the surface takes the form

$$z = \Phi_1(y)\,\Psi_1\{x - f_1(y)\} = \Phi_2(x)\,\Psi_2\{y - f_2(x)\} \quad \text{.....(i)},$$

or if small letters be written for logarithms of capitals

$$\log z = \phi_1(y) + \psi_1\{x - f_1(y)\} = \phi_2(x) + \psi_2\{y - f_2(x)\} \quad \text{.....(ii)}.$$

If we differentiate this first with regard to x and then with regard to y, we shall free ourselves from the functions ϕ_1 and ϕ_2. We have

$$-\frac{1}{z}\frac{\partial^2 z}{\partial x\,\partial y} + \frac{1}{z^2}\frac{\partial z}{\partial x}\frac{\partial z}{\partial y} = f_1'(y)\,\psi_1''\{x - f_1(y)\} = f_2'(x)\,\psi_2''\{y - f_2(x)\} \quad \text{.....(iii)}$$

as our general functional equation for the special case of homoscedasticity.

Case (*a*). Let us suppose the position of the x-array to be independent of y, i.e. zero correlation when we consider x as a function of y. Hence $f_1(y) = \text{constant} = c_1$, and $f_1'(y) = 0$. Accordingly either

$$f_2'(x) = 0 \quad \text{or} \quad \psi_2''\{y - f_2(x)\} = 0.$$

(i) In the former case $f_2(x) = \text{constant} = c_2$. Thus equation (i) gives us

$$z = \Phi_1(y)\,\Psi_1(x - c_1) = \Phi_2(x)\,\Psi_2(y - c_2),$$

or

$$z = \chi_1(y) \times \chi_2(x),$$

i.e. the product of two independent functions of x and y respectively. This is of course the most general case of no correlation or completely independent probability.

(ii) Now in the second case

$$\psi_2''\{y - f_2(x)\} = 0,$$

or

$$\psi_2\{y - f_2(x)\} = c_3\{y - f_2(x)\} + c_4,$$

and we treat $f_2(x)$ as arbitrary.

Accordingly we have

$$z = \Phi_1(y)\,\Psi_1(x - c_1) = e^{c_3\{y - f_2(x)\} + c_4}\,\Phi_2(x);$$

obviously this is satisfied if

$$\Phi_1(y) = e^{c_3 y + c_4}, \text{ and } \Psi_1(x - c_1) = e^{-c_3 f_2(x)}\,\Phi_2(x).$$

Thus we can look at the result in either of two ways:

1. The case is a special case of the preceding one, z is an arbitrary function of x, multiplied by an exponential curve in y, i.e. there is independent probability; or

2. We can, while the regression of x on y is a horizontal line, consider the regression of y on x to take any value we like. But this is only possible when there is homoscedasticity provided the arrays of y for a given x are all exponential curves. The arrays of x for a given y will be quite arbitrary. Accordingly: There is no system of homoscedastic arrays both ways with one regression zero, and the

other arbitrary, i.e. $\eta_{x,y} = 0$ and $\eta_{y,x}$ arbitrary, unless the arrays one way are exponential curves.

Since, however, all exponential curves are similar curves wherever their origins be taken, this case can be considered a case of independent probability.

We can therefore assert that it is impossible for one correlation-ratio $\eta_{x,y}$ to be zero and the other finite unless one series of arrays is heteroscedastic, the exception being when one series of arrays is an exponential curve.

Of course in any frequency surface we cannot have an infinite area for any array, and thus when we speak of an exponential curve as an array we must understand a portion of this curve from a finite value of, say, y to $+\infty$ or $-\infty$ as the case may be.

Case (*b*). Let us suppose both regressions to be linear, i.e. $f_1(y) = m_1 y + c_1$, $f_2(x) = m_2 x + c_2$. Then equation (iii) takes the form:

$$m_1 \psi_1''(x - m_1 y - c_1) = m_2 \psi_2''(y - m_2 x - c_2) \quad \text{.....(iv).}$$

Differentiate this relation with respect to x and with respect to y, and we have

$$\left.\begin{aligned} m_1\, \psi_1'''(x - m_1 y - c_1) &= -m_2^2\, \psi_2'''(y - m_2 x - c_2) \\ -m_1^2 \psi_1'''(x - m_1 y - c_1) &= m_2\, \psi_2'''(y - m_2 x - c_2) \end{aligned}\right\} \quad \text{.....(v).}$$

Accordingly it is needful either that

1. $m_1 = 0$ and $m_2 = 0$ or $\psi_2'''(y - m_2 x - c_2) = 0$.
2. $m_2 = 0$ and $m_1 = 0$ or $\psi_1'''(x - m_1 y - c_1) = 0$.

These two cases fall into the last case (*a*).

3. $\psi_1'''(x - m_1 y - c_1) = 0$, and $\psi_2'''(y - m_2 x - c_2) = 0$, or
4. $m_1 m_2 = 1$.

(i) Let us consider first the third possibility.

We have at once

$$\psi_1(x - m_1 y - c_1) = \alpha_0 (x - m_1 y - c_1)^2 + \alpha_1 (x - m_1 y - c_1) + \alpha_2,$$

$$\psi_2(y - m_2 x - c_2) = \gamma_0 (y - m_2 x - c_2)^2 + \gamma_1 (y - m_2 x - c_2) + \gamma_2.$$

But from (iv) we have

$$\alpha_0 m_1 = \gamma_0 m_2,$$

or we may put $\qquad \alpha_0 = \lambda_0 m_2, \quad \gamma_0 = \lambda_0 m_1,$

where λ_0 is a new constant.

Equation (ii) may be written as

$$\psi_1\{x - f_1(y)\} - \psi_2\{y - f_2(x)\} = \phi_2(x) - \phi_1(y).$$

Hence we find, if ρ be a constant,

$$\phi_1(y) = \lambda_0 \{m_1(1 - m_1 m_2) y^2 - 2m_1(c_2 + m_2 c_1) y + m_1 c_2^2\} + (\gamma_1 + m_1 \alpha_1) y - \gamma_1 c_2 + \gamma_2 + \rho,$$

$$\phi_2(x) = \lambda_2 \{m_2(1 - m_1 m_2) x^2 - 2m_2(c_1 + m_1 c_2) x + m_2 c_1^2\} + (\alpha_1 + m_2 \gamma_1) x - \alpha_1 c_1 + \alpha_2 + \rho.$$

Accordingly

$$\log z = \phi_2(x) + \psi_2(y - m_2x - c_2)$$
$$= \lambda_2\{m_2x^2 - 2m_1m_2xy + m_1y^2 - 2(m_2c_1x + m_1c_2y) + m_2c_1^2 + m_1c_2^2\} + \alpha_1x + \gamma_1y - \gamma_1c_2 - \alpha_1c_1 + \alpha_2 + \gamma_2 + \rho,$$

or, changing the origin,

$$z = z_0 e^{\lambda_0(m_2x'^2 - 2m_1m_2x'y' + m_1y'^2)},$$

which will be the usual form of the normal correlation surface, if we take

$$\lambda_0 = -\frac{1}{r(1-r^2)}\frac{1}{\sigma_x}\frac{1}{\sigma_y}, \quad m_1 = r\frac{\sigma_x}{\sigma_y}, \quad m_2 = r\frac{\sigma_y}{\sigma_x}.$$

The latter results follow at once if we remember that $y = m_2x + c_2, x = m_1y + c_1$ are the two regression lines.

Hence unless 4 holds, i.e. $m_1m_2 = 1$, linear homoscedastic regression for both variates is only possible in the case of the normal frequency surface.

(ii) Suppose $m_1m_2 = 1$, then equation (iv) gives us

$$\psi_1''(x - m_1y - c_1) = \frac{1}{m_1^2}\psi_2''\left(y - \frac{x}{m_1} - c_2\right),$$

or, since each of these is a function of $x - m_1y$, we have

$$\psi_1''(x - m_1y - c_1) = \frac{1}{m_1^2}\psi_2''\left(y - \frac{x}{m_1} - c_2\right) = \psi''(x - m_1y).$$

Hence by integration with regard to $x - m_1y$,

$$\psi_1(x - m_1y - c_1) = \psi(x - m_1y) + \alpha_1(x - m_1y) + \alpha_2,$$
$$\psi_2\left(y - \frac{1}{m_1}x - c_2\right) = \psi(x - m_1y) + \gamma_1(x - m_1y) + \gamma_2.$$

But $$\psi_1(x - m_1y - c_1) - \psi_2\left(y - \frac{1}{m_1}x - c_2\right) = \phi_2(x) - \phi_1(y)$$
$$= \{(\alpha_1 - \gamma_1)x + \alpha_2 + \epsilon\} - \{(\alpha_1 - \gamma_1)m_1y + \gamma_2 + \epsilon\},$$

where α_1, α_2, γ_1, γ_2 and ϵ are arbitrary constants.

Hence
$$\phi_1(y) = (\alpha_1 - \gamma_1)m_1y + \gamma_2 + \epsilon,$$
$$\phi_2(x) = (\alpha_1 - \gamma_1)x + \alpha_2 + \epsilon.$$

Now

$$\log z = \phi_1(y) + \psi_1(x - m_1y - c_1) = \psi(x - m_1y) + \alpha_1x - \gamma_1m_1y + \alpha_2 + \gamma_2 + \epsilon.$$

Accordingly
$$z = \Psi(x - m_1y)\, e^{\alpha_1x - \gamma_1m_1y} \quad \text{.....(vi)},$$

where the constant is thrown into the function Ψ.

Now let us consider the means of the arrays $\tilde{y}_x$ and $\tilde{x}_y$ respectively.

$$\tilde{y}_x = \int_{y_1}^{y_2} zy\,dy \Big/ \int_{y_1}^{y_2} z\,dy, \quad \tilde{x}_y = \int_{x_1}^{x_2} zx\,dx \Big/ \int_{x_1}^{x_2} z\,dx.$$

Here y_1, y_2 denote the limits of the x-arrays of y's and may be functions of x, and x_1, x_2 may similarly be functions of y.

Let us write $$u = x - m_1 y.$$

For x constant, $y = \dfrac{x-u}{m_1}$, $\quad dy = -\dfrac{1}{m_1}\,du$, and accordingly

$$\tilde{y}_x = \int_{x-m_1y_1}^{x-m_1y_2} \psi(u)\,\frac{x-u}{m_1}\,e^{\gamma_1 u + (\alpha_1-\gamma_1)x}\left(\frac{du}{m_1}\right) \Big/ \int_{x-m_1y_1}^{x-m_1y_2} \psi(u)\,e^{\gamma_1 u+(\alpha_1-\gamma_1)x}\left(\frac{du}{m_1}\right)$$

$$= \frac{x}{m_1} - \frac{1}{m_1}\int_{x-m_1y_2}^{x-m_1y_1} \psi(u)\,ue^{\gamma_1 u}\,du \Big/ \int_{x-m_1y_2}^{x-m_1y_1} \psi(u)\,e^{\gamma_1 u}\,du \quad \text{.....(vii).}$$

Again for y constant, $x = u + m_1 y$, $dx = du$, and we have

$$\tilde{x}_y = \int_{x_1-m_1y}^{x_2-m_1y} \psi(u)\,(u+m_1y)\,e^{\alpha_1 u + m_1(\alpha_1-\gamma_1)y}\,du \Big/ \int_{x_1-m_1y}^{x_2-m_1y} \psi(u)\,e^{\alpha_1 u+m_1(\alpha_1-\gamma_1)y}\,du$$

$$= m_1 y + \int_{x_1-m_1y}^{x_2-m_1y} \psi(u)\,ue^{\alpha_1 u}\,du \Big/ \int_{x_1-m_1y}^{x_2-m_1y} \psi(u)\,e^{\alpha_1 u}\,du \quad \text{.....(viii).}$$

Now it appears improbable, if not impossible, for the ratio of integrals in (vii) to be independent of x, and the ratio of integrals in (viii) to be independent of y, unless the limits themselves are independent respectively of x and y, i.e. unless the arrays are integrated between *parallels* to the regression lines. Further these regression lines must coincide, for otherwise, being parallel, their point of intersection, the common mean of the two variates, would be at infinity, which is not possible in practical statistics. Accordingly the regression line being

$$\tilde{x}_y = m_1 y + c_1,$$

we have

$$c_1 = \frac{\int_{a_1}^{b_1} \psi(u)\,ue^{\alpha_1 u}\,du}{\int_{a_1}^{b_1} \psi(u)\,e^{\alpha_1 u}\,du}$$

$$= \frac{\int_{a_1}^{b_1} \psi(u)\,ue^{\gamma_1 u}\,du}{\int_{a_1}^{b_1} \psi(u)\,e^{\gamma_1 u}\,du}.$$

This again seems very improbable, if not impossible, unless $\alpha_1 = \gamma_1$. In this case the functional equation (vi) reduces to

$$z = \Psi(x - m_1 y),$$

or is that of a ruled surface. The volume of such a surface cannot be finite, unless the ruled surface is *limited*. But if the limits be taken to give a series of parallel sections one way, they cannot do so the other, unless all the frequency collapses into the plane through the regression line perpendicular to the x, y plane, i.e. unless the correlation be perfect.

We have retained this longer and less satisfactory method of approach, because we have been looking at the whole subject from the standpoint of functional analysis. The discussion is quite straightforward, however, from the standpoint of correlation. For in that case we have

$$m_1 = r\sigma_x/\sigma_y, \quad m_2 = r\sigma_y/\sigma_x,$$

and accordingly $m_1 m_2 = 1$ involves $r^2 = 1$ or perfect correlation. The mean square deviations from the regression lines $\sigma_x^2(1-r^2)$ and $\sigma_y^2(1-r^2)$ are both zero, or all points fall into the plane through the regression line, as the functional analysis suggests rather than rigidly proves.

Case (c). Further generalisations of homoscedastic surfaces.

Returning to equation (iii), p. 79 let us take logarithmic differentials of it with regard to x and y. We have

$$\frac{\psi_1'''\{x-f_1(y)\}}{\psi_1''\{x-f_1(y)\}} = \frac{f_2''(x)}{f_2'(x)} - f_2'(x)\frac{\psi_2'''\{y-f_2(x)\}}{\psi_2''\{y-f_2(x)\}} \quad \text{.....(ix),}$$

and

$$\frac{\psi_2'''\{y-f_2(x)\}}{\psi_2''\{y-f_2(x)\}} = \frac{f_1''(y)}{f_1'(y)} - f_1'(y)\frac{\psi_1'''\{x-f_1(y)\}}{\psi_1''\{x-f_1(y)\}} \quad \text{.....(ix bis).}$$

Let us make the limited assumption now that

$$\frac{\psi_1'''\{x-f_1(y)\}}{\psi_1''\{x-f_1(y)\}} = \text{a constant} = \lambda_1.$$

Then it follows that the right-hand side of the first equation can contain no y and the left-hand side of the second equation no x. Accordingly

$$\frac{\psi_2'''\{y-f_2(x)\}}{\psi_2''\{y-f_2(x)\}} = \text{a constant} = \lambda_2 \text{ say.}$$

Thus we have

$$\frac{f_2''(x)}{f_2'(x)} - \lambda_2 f_2'(x) = \lambda_1,$$

and

$$\frac{f_1''(y)}{f_1'(y)} - \lambda_1 f_1'(y) = \lambda_2.$$

The solutions of these equations are easily found to be

$$\tilde{y}_x = f_2(x) = c_2 - \frac{1}{\lambda_2}\log\left(1 - \frac{\lambda_2}{\lambda_1} m_2 e^{\lambda_1 x}\right) \quad \text{.....(x),}$$

$$\tilde{x}_y = f_1(y) = c_1 - \frac{1}{\lambda_1}\log\left(1 - \frac{\lambda_1}{\lambda_2} m_1 e^{\lambda_2 y}\right) \quad \text{.....(x bis),}$$

where c_1, c_2, m_1, m_2 are arbitrary constants, and these equations give the skew regression lines.

The equations

$$\psi_2'''\{y-f_2(x)\} = \lambda_2 \psi_2''\{y-f_2(x)\},$$

and

$$\psi_1'''\{x-f_1(y)\} = \lambda_1 \psi_1''\{x-f_1(y)\},$$

lead at once to

$$\psi_2\{y-f_2(x)\} = \frac{c_2'}{\lambda_2^2} e^{\lambda_2\{y-f_2(x)\}} + c_2''\{y-f_2(x)\} + c_2''',$$

$$\psi_1\{x-f_1(y)\} = \frac{c_1'}{\lambda_1^2} e^{\lambda_1\{x-f_1(y)\}} + c_1''\{x-f_1(y)\} + c_1''',$$

where the c_1', c_2', c_1'', c_2'', c_1''', c_2''' are arbitrary constants. But in order to satisfy the fundamental equation

$$f_1'(y)\psi_1''\{x-f_1(y)\} = f_2'(x)\psi_2''\{y-f_2(x)\},$$

it is needful to take

$$m_1 c_1' e^{-c_1\lambda_1} = m_2 c_2' e^{-c_2\lambda_2},$$

or
$$c_1' = \frac{\gamma e^{c_1\lambda_1}}{m_1}, \quad c_2' = \frac{\gamma e^{c_2\lambda_2}}{m_2},$$

where γ is an arbitrary constant. Thus

$$\psi_1\{x - f_1(y)\} = \frac{\gamma}{\lambda_1^2 m_1} e^{\lambda_1\{x - f_1(y) + c_1\}} + c_1''\{x - f_1(y)\} + c_1''',$$

$$\psi_2\{y - f_2(x)\} = \frac{\gamma}{\lambda_2^2 m_2} e^{\lambda_2\{y - f_2(x) + c_2\}} + c_2''\{y - f_2(x)\} + c_2'''.$$

Or, substituting for $f_1(y)$ and $f_2(x)$,

$$\psi_1\{x - f_1(y)\} = \frac{\gamma}{\lambda_1^2 m_1} e^{\lambda_1 x}\left(1 - \frac{\lambda_1}{\lambda_2} m_1 e^{\lambda_2 y}\right) + c_1''\left\{x + \frac{1}{\lambda_1}\log\left(1 - \frac{\lambda_1}{\lambda_2} m_1 e^{\lambda_2 y}\right)\right\} + c_1^{\text{iv}},$$

$$\psi_2\{y - f_2(x)\} = \frac{\gamma}{\lambda_2^2 m_2} e^{\lambda_2 y}\left(1 - \frac{\lambda_2}{\lambda_1} m_2 e^{\lambda_1 x}\right) + c_2''\left\{y + \frac{1}{\lambda_2}\log\left(1 - \frac{\lambda_2}{\lambda_1} m_2 e^{\lambda_1 x}\right)\right\} + c_2^{\text{iv}},$$

where c_1^{iv} and c_2^{iv} are new arbitrary constants.

Accordingly

$$\begin{aligned}\phi_2(x) - \phi_1(y) &= \psi_1\{x - f_1(y)\} - \psi_2\{y - f_2(x)\} \\ &= \left\{\frac{\gamma}{\lambda_1^2 m_1} e^{\lambda_1 x} + c_1'' x - \frac{c_2''}{\lambda_2}\log\left(1 - \frac{\lambda_2}{\lambda_1} m_2 e^{\lambda_1 x}\right) + c_1^{\text{iv}}\right\} \\ &= \left\{\frac{\gamma}{\lambda_2^2 m_2} e^{\lambda_2 y} + c_2'' y - \frac{c_1''}{\lambda_1}\log\left(1 - \frac{\lambda_1}{\lambda_2} m_1 e^{\lambda_2 y}\right) + c_2^{\text{iv}}\right\}.\end{aligned}$$

Hence we find

$$\phi_2(x) = \frac{\gamma}{\lambda_1^2 m_1} e^{\lambda_1 x} + c_1'' x - \frac{c_2''}{\lambda_2}\log\left(1 - \frac{\lambda_2 m_2}{\lambda_1} e^{\lambda_1 x}\right) + c_1^{\text{v}},$$

$$\phi_1(y) = \frac{\gamma}{\lambda_2^2 m_2} e^{\lambda_2 y} + c_2'' y - \frac{c_1''}{\lambda_1}\log\left(1 - \frac{\lambda_1 m_1}{\lambda_2} e^{\lambda_2 y}\right) + c_2^{\text{v}},$$

where c_1^{v} and c_2^{v} are arbitrary constants.

Thus we reach as the equation to our frequency surface

$$\begin{aligned}\log z &= \phi_1(y) + \psi_1\{x - f_1(y)\} \\ &= -\frac{\gamma}{\lambda_1\lambda_2} e^{\lambda_1 x + \lambda_2 y} + \frac{\gamma}{\lambda_1^2 m_1} e^{\lambda_1 x} + \frac{\gamma}{\lambda_2^2 m_2} e^{\lambda_2 y} + c_1'' x + c_2'' y + c_3'',\end{aligned}$$

or
$$z = z_0 e^{-\left(\frac{\gamma}{\lambda_1\lambda_2} e^{\lambda_1 x + \lambda_2 y} - \frac{\gamma}{\lambda_1^2 m_1} e^{\lambda_1 x} - \frac{\gamma}{\lambda_2^2 m_2} e^{\lambda_2 y} - c_1'' x - c_2'' y\right)} \quad \text{.....(xi).}$$

This is the most general frequency surface so far obtained for bivariate homoscedasticity. It will be seen that the array-curves are curves closely allied to Makeham curves and will be actually such if $c_1'' = c_2'' = 0$; the regression curves are both of the same type as the logarithmico-exponential curves of equations (x) and (x bis) of p. 83.

If we make the transformation

$$X = e^{\lambda_1 x}, \quad Y = e^{\lambda_2 y},$$

we have

$$\frac{A_1}{\tilde{X}_y} = \frac{1}{\lambda_1 m_1} - \frac{Y}{\lambda_2}, \quad \frac{A_2}{\tilde{Y}_x} = \frac{1}{\lambda_2 m_2} - \frac{X}{\lambda_1},$$

$$z = z_0' \, X^{n_1} Y^{n_2} e^{-\gamma\left(\frac{1}{\lambda_1 m_1} - \frac{Y}{\lambda_2}\right)\left(\frac{1}{\lambda_2 m_2} - \frac{X}{\lambda_1}\right)},$$

for the regression curves and the equation to the surface. The regression curves are now seen to be rectangular hyperbolas and the array-curves are curves of Pearson's Type III.

Special Case (i). Suppose $\lambda_1/\lambda_2 = q$ and $\lambda_2 = 0$.

Then the regression curves become

$$\tilde{y}_x = a_2 + m_2 q^2 x, \quad \tilde{x}_y = a_1 + \frac{m_1}{q} y,$$

where a_1 and a_2 are arbitrary constants. Again

$$z = z_0' e^{-\frac{1}{2}\gamma\left\{\left(q - \frac{1}{m_1}\right)x^2 + 2xy + \left(\frac{1}{q} - \frac{1}{m_2}\right)y^2 + g_1 x + g_2 y\right\}},$$

where g_1 and g_2 are arbitrary constants measuring only the position of the origin. Thus the surface becomes the normal surface and the regressions are linear as before. It is clear, therefore, that our surface, however complicated its formula looks, cannot in form depart widely from the normal surface when λ_1 and λ_2 are not large.

Special Case (ii). Let us suppose only $\lambda_1 = 0$.

Returning to equations (x) and (x bis) and putting $\lambda_1 = 0$, we readily find

$$\tilde{x}_y = a_2' + \frac{m_2}{\lambda_2} e^{\lambda_2 y}, \quad \tilde{y}_x = a_1' - \frac{1}{\lambda_2} \log (1 - m_1 \lambda_2 x),$$

where a_1' and a_2' are arbitrary constants. Thus one regression line is an exponential curve and the other a logarithmic curve.

(3) Linear Regression and Linear Variation for arrays both ways.

Returning to our general equation on p. 78:

$$z = \Phi_1(y)\, \Psi_1[\{x - f_1(y)\}\, F_1(y)] = \Phi_2(x)\, \Psi_2[\{y - f_2(x)\}\, F_2(x)] \quad \ldots(\text{xii}),$$

we must write if the regression be linear both ways

$$f_1(y) = m_1 y + c_1, \quad f_2(x) = m_2 x + c_2,$$

and if the arrays are heteroscedastic but linear

$$\frac{1}{F_1(y)} = \lambda_1 (y + a_1), \quad \frac{1}{F_2(x)} = \lambda_2 (x + a_2).$$

Hence we can put

$$\{x - f_1(y)\}\, F_1(y)$$

in the form

$$\frac{x - m_1 y - c_1}{\lambda_1 (y + a_1)} = \frac{1}{\lambda_1} \frac{x - c_1 + m_1 a_1}{y + a_1} - \frac{m_1}{\lambda_1} = \frac{1}{\lambda_1} \frac{x + g_1}{y + a_1} - \frac{m_1}{\lambda_1},$$

where $g_1 = m_1 a_1 - c_1$.

Similarly $$\{y - f_2(x)\} F_2(x) = \frac{1}{\lambda_2} \frac{y + g_2}{x + a_2} - \frac{m_2}{\lambda_2},$$

where $g_2 = m_2 a_2 - c_2$.

Clearly the λ_1 and λ_2 may be absorbed into the functions Ψ_1 and Ψ_2; and if we write

$$u + m_1 = (x + g_1)/(y + a_1), \quad v + m_2 = (y + g_2)/(x + a_2) \quad \ldots\ldots\ldots\text{(xiii)},$$

we have on taking the logarithmic differential of (xii) first with regard to x and then with regard to y

$$\frac{1}{(y + a_1)^2} \left\{ \psi_1' \left(\frac{x + g_1}{y + a_1} - m_1 \right) + \frac{x + g_1}{y + a_1} \psi_1'' \left(\frac{x + g_1}{y + a_1} - m_1 \right) \right\}$$
$$= \frac{1}{(x + a_2)^2} \left\{ \psi_2' \left(\frac{y + g_2}{x + a_2} - m_2 \right) + \frac{y + g_2}{x + a_2} \psi_2'' \left(\frac{y + g_2}{x + a_2} - m_2 \right) \right\},$$

or, since

$$y + a_1 = \frac{g_2 - a_1 + (g_1 - a_2)(v + m_2)}{(u + m_1)(v + m_2) - 1},$$

$$x + a_2 = \frac{g_1 - a_2 + (g_2 - a_1)(u + m_1)}{(u + m_1)(v + m_2) - 1},$$

we have

$$\{(g_1 - a_2) + (g_2 - a_1)(u + m_1)\}^2 \{\psi_1'(u) + (u + m_1)\psi_1''(u)\}$$
$$= \{(g_2 - a_1) + (g_1 - a_2)(v + m_2)\}^2 \{\psi_2'(v) + (v + m_2)\psi_2''(v)\} \ldots\text{(xiii bis)},$$

the only exception being when both

$$g_1 - a_2 = 0 \text{ and } g_2 - a_1 = 0,$$

a case we shall consider later.

But since the left-hand side is a function of u only and the right of v only, it follows that both must be constant or $= q(g_1 - a_2)(g_2 - a_1)$ say.

Hence $$\frac{d}{du} \{\psi_1'(u)(u + m_1)\} = \frac{q(g_1 - a_2)(g_2 - a_1)}{\{g_1 - a_2 + (g_2 - a_1)(u + m_1)\}^2},$$

or $$\psi_1'(u) = \frac{p_1 + q}{u + m_1} - \frac{q}{u + m_1} \cdot \frac{q_1 - a_2}{g_1 - a_2 + (g_2 - a_1)(u + m_1)}.$$

Thus $$\Psi(u) = c(u + m_1)^{p_1} \{g_1 - a_2 + (g_2 - a_1)(u + m_1)\}^q,$$

where c and p_1 are constants.

Transferring to x and y we have

$$\Psi_1 \left(\frac{x + g_1}{y + a_1} - m_1 \right) = c \frac{(x + g_1)^{p_1}}{(y + a_1)^{p_1 + q}} \{(g_1 - a_2)(y + a_1) + (g_2 - a_1)(x + a_2)\}^q.$$

Similarly

$$\Psi_2 \left(\frac{y + g_2}{x + a_2} - m_2 \right) = c' \frac{(y + g_2)^{p_2}}{(x + a_2)^{p_2 + q}} \{(g_1 - a_2)(y + a_1) + (g_2 - a_1)(x + a_2)\}^q.$$

Hence returning to the fundamental equation

$$\phi_1(y) - \phi_2(x) = \log c' + p_2 \log(y + g_2) + (p_1 + q) \log(x + a_1)$$
$$- \log c - p_1 \log(x + g_1) - (p_2 + q) \log(y + a_2).$$

Accordingly

$$\phi_1(y) = \log c'' + p_2 \log(y+g_2) + (p_1+q)\log(x+a_1),$$
$$\phi_2(x) = \log c'' + p_1 \log(x+g_1) + (p_2+q)\log(y+a_2),$$

and $$\log z = \phi_1(y) + \psi_1\left(\frac{x+g_1}{y+a_1} - m_1\right)$$
$$= \log c^{iv} + p_2 \log(y+g_2) + p_1 \log(x+g_1) + q \log\{(g_1-a_2)(y+a_1) + (g_2-a_1)(x+a_2)\},$$

or $$z = z_0 (x+g_1)^{p_1} (y+g_2)^{p_2} \{(g_1-a_2)(y+a_1) + (g_2-a_1)(x+a_2)\}^q \quad \ldots(\text{xiv}).$$

This is the most general equation to frequency surfaces having linear regression both ways and linear heteroscedasticity. Clearly the arrays both ways are Pearson curves of Type I.

We can now consider special cases.

Case (i). Suppose $g_1 - a_2 = 0$.

We may write (xiv) as

$$z = z_0 (x+g_1)^{p_1} (y+g_2)^{p_2} (g_2-a_1)^q (x+a_2)^q \left\{1 + \frac{(g_1-a_2)(y+a_1)}{(g_2-a_1)(x+a_2)}\right\}^q.$$

Assume $$p_1' = p_1 + q, \quad z_0 (g_2-a_1)^q = z_0', \quad q = q' \frac{(g_2-a_1)}{(g_1-a_2)}.$$

Hence $$z = z_0' (x+g_1)^{p_1'} (y+g_2)^{p_2} e^{q' \left(\frac{y+a_1}{x+g_1}\right)}.$$

Similarly if $g_2 - a_1 = 0$,

$$z = z_0'' (x+g_1)^{p_1} (y+g_2)^{p_2'} e^{q'' \left(\frac{x+a_2}{y+g_2}\right)}.$$

These special cases have the arrays one way Pearson curves of Type III, and the arrays the other way Pearson curves of Type VI. The physical interpretation of $g_1 - a_2 = 0$ appears to be that the variability of the array at the bounding border of the surface ($x = -g_1$) is zero. The above results coincide with what one obtains by taking $g_1 = a_2$ (or $g_2 = a_1$) in equation (xiii bis) and then proceeding to the integrations.

Case (ii). Let us suppose both $g_1 = a_2$ and $g_2 = a_1$.

Then returning to the equations (xiii) on p. 86 we see that

$$u + m_1 = 1/(v + m_2),$$

or u and v are not independent variables. Let us put as before $u + m_1 = \frac{x+g_1}{y+g_2}$, then

$$\phi_1(y) + \psi_1(u) = \phi_2\{(y+g_2)(u+m_1) - g_1\} + \psi_2\left(\frac{1}{u+m_1} - m_2\right).$$

Differentiate first with regard to y and then with regard to u and we have

$$0 = \phi_2'\{(y+g_2)(u+m_1) - g_1\} + (u+m_1)(y+g_2)\,\phi_2''\{(y+g_2)(u+m_1) - g_1\}.$$

Or, writing $$x = (y+g_2)(u+m_1) - g_1,$$
$$\phi_2'(x) + (x+g_1)\,\phi_2''(x) = 0.$$

Accordingly $$\phi_2(x) = c_2 + p_2 \log(x + g_1),$$

where c_2 and p_2 are constants.

Similarly we must have

$$\phi_1(y) = c_1 + p_1 \log(y + g_2) = c_1 + p_1 \log(x + g_1) - p_1 \log(u + m_1).$$

Hence

$$\log z = c_1 + p_1 \log(x + g_1) - p_1 \log(u + m_1) + \psi_1(u)$$
$$= c_2 + p_2 \log(x + g_2) + \psi_2\left(\frac{1}{u + m_1} - m_2\right).$$

Since x and u are independent, we must have $p_1 = p_2$ and make ψ_2 an arbitrary function of $(x + g_1)/(y + g_2)$.

Accordingly $$z = z_0 (x + g_1)^{p_1} F_1\left(\frac{x + g_1}{y + g_2}\right),$$

or, as we may also write it,

$$= z_0 (y + g_2)^{p_1} F_2\left(\frac{y + g_2}{x + g_1}\right),$$

or again symmetrically

$$z = z_0 \{(x + g_1)(y + g_2)\}^{\frac{1}{2}p_1} F_3\left(\frac{x + g_1}{y + g_2}\right).$$

This form suggests that in most cases bounding lines of the surface, i.e. $x = -g_1$ and $y = -g_2$ give arrays of zero variability.

If we write $$x + g_1 = R\cos\theta, \quad y + g_2 = R\sin\theta,$$
$$z = z_0' R^{p_1} (\sin 2\theta)^{p_1} F_3'(\cot\theta).$$

Sections by the planes $\theta =$ constant are therefore high order parabolae and will not provide finite frequency unless p_1 be negative and less than unity and the surface be supposed bounded away from the origin. There seem difficulties about this surface from the standpoint of practical statistics.

NATURAL SELECTION AND THE AGE AND AREA THEORY OF DR J. C. WILLIS.

By EGON S. PEARSON, B.A.

PART I.

(1) One of the lines of attack most frequently directed against Darwin's theory of Natural Selection is concerned with the question of adaptation; it is claimed that it is never possible to show satisfactorily why the peculiar features which constitute its specific or generic character render a particular form of animal or plant life specially suited for its environment, nor how it could have reached its present form by a gradual process of modification under the action of natural selection. After a study *in situ* of the flora of Ceylon, Dr Willis was convinced of the justification of this line of attack*, but he has also found that a statistical analysis of the figures of areal distribution and of the frequencies of species and genera in both Ceylon and New Zealand leads to results which he believes are even more damaging to Darwin's hypothesis, and call for a new theory of evolution†. It is proposed to discuss here certain aspects of this problem of statistical frequencies, rather than the question of special adaptation. Just as any theory of inheritance, whether on Mendelian or other lines, must stand or fall in the long run according as the statistical relations which it predicts are or are not in accordance with the statistics of actual observation‡, so any theory of evolution which leads to results inconsistent with the statistical results must necessarily be suspect.

At the commencement of any statistical inquiry into the origin of species we are confronted with uncertainty as to the value of our units of grouping. The grouping of varieties into species, of species into genera and of genera into families is admittedly very arbitrary, and the classification may change according to the views of the individual systematist. Dr Willis of course does not believe that there is any definite distinction underlying these groups§, but presumably considers the divisions to form a useful if somewhat rough scheme of classification of the distinct forms created by mutations. In the same way, on the Darwinian theory, the grouping may be considered to give a rough measure of the degree of

* See for example a series of papers published in the *Annals of the Royal Botanic Gardens, Peradeniya*, Vols. III—V. 1906—1911.

† *Phil. Trans.* Vol. 206 B. p. 307; a series of papers in the *Annals of Botany*, Vols. XXX—XXXV. 1916—1921; and finally *Age and Area*, Cambridge University Press, 1922.

‡ We may test, for example, whether the observed coefficients of correlation between offspring and ancestry agree with the correlation of somatic characters predicted on the Mendelian theory.

§ See, for example, his remarks in *Phil. Trans.* Vol. 206 B. pp. 330, 331.

relationship between allied forms. Yet if these units are employed in statistical work, the possibility of spurious results arising from the method of classification must never be forgotten.

Before discussing some of the more important aspects of Dr Willis's interpretation, it is desirable to consider what form of recurrent relations we might expect to find among groups of species combined to provide average results, if evolution had followed the general lines suggested by Darwin. In answering his critics on the question of endemics*, Dr Willis suggests that the supporters of the theory of Natural Selection can be divided into two groups; those who believe that the endemic species "are local adaptations suited expressly to the spots in which they occur"; and those who take "the general position that endemics are mostly the relics of pre-existing floras†." But surely both these points of view are contained in the theory of Natural Selection, and not one to the exclusion of the other? Darwin himself imagined that a new variety or species was evolved within a comparatively small area, and gradually spread outwards from this. We find him writing in the *Origin of Species*‡: "Each newly formed variety would generally be at first local as seems to be the common rule with varieties in a state of nature; so that similarly modified individuals would soon exist in a small body together and would often breed together. If the new variety were successful in its battle for life, it would slowly spread from a central district, competing with and conquering the unchanged individuals on the margin of an ever increasing circle." If we accept this hypothesis we must suppose that among the species endemic to a country there will not only be relics suffering extinction, but at the same time many new forms which in Dr Willis's words are "too young" to have attained a greater dispersal, and which have been derived either from the species in the same genus or family still found alongside them, or from other allied species now extinct in the country. The islands of New Zealand, for example, have been isolated from the rest of the world for a very long time; here, new forms will have developed during the period of isolation, and it is clearly among the endemic species that we must look to find them§. It may be a problem of great difficulty to determine in a particular endemic flora the relative proportions of the species which are dying out and of those which are young and progressive‖, but the idea that a large number of the endemic forms are of more recent origin than the wide forms cannot be considered inconsistent with the theory of evolution by Natural Selection.

* A species "endemic" to a country is one found only in that country, while a "wide" species is one found also elsewhere in the world; the terms are of course purely relative to the country or district under consideration.

† *Age and Area*, pp. 87—89.

‡ *Origin of Species*, 6th Edition, Chapter IV. p. 113.

§ The fact, which will be referred to below, that at Cook's Strait, dividing the North and South Islands, there is a marked discontinuity in the endemic but not in the wide distribution, certainly suggests that many of the endemic species have a shorter history in New Zealand than the wides.

‖ Dr Willis suggests that "perhaps 1 % of the grand total of endemic species are relics," *Age and Area*, p. 108.

In any species there is necessarily a correlation between age and area; age considered as a pure time factor tends to increase dispersal, in so far as all species if not checked by environmental conditions will spread outwards. On the other hand the effect of the various forces of environment which may have been of great importance in determining the history of the individual species has been selective, so that the conditions which have checked the spread of one species may perhaps have encouraged that of another species in the same genus. It follows that when we compare the mean area covered by each of two groups of species having not too different an origin, we should expect to find that the older group covers a larger area than the younger. Again, without adopting any theory of mutation, we should expect that on the average the longer a species has been in existence, the more new forms will have developed from it. Darwin has expressed this point very clearly. "The mere lapse of time," he wrote*, "does nothing either for or against natural selection. I state this because it has been erroneously asserted that the element of time has been assumed by me to play an all important part in modifying species, as if all forms of life were necessarily undergoing change through some innate law. Lapse of time is only so far important, and its importance in this respect is great, that it gives a better chance of beneficial variations arising, and of their being selected, accumulated and fixed."

All, indeed, who believe that natural selection has played a fundamental part in determining the relative dispersal and distribution of species, whether they hold with Darwin that the origin of species has been through a gradual process of divergence, or with de Vries that it has been by mutation, must be prepared to find in comparing average figures that the age factor is emphasised through the cancelling out of the selective factors. But Dr Willis looks at the problem from an opposite point of view; his theory supposes that it is the principles of Age and Area and of Size and Space† which are fundamental, and that since they are obscured in individual cases by numerous modifying but less important causes, a comparison of average distributions is essential to demonstrate their existence. He claims that an examination of the statistical results which he has collected provides ample proof of the necessity for, and the correctness of, his hypothesis; yet I venture to think that the problem of discriminating between the soundness of different theories of evolution, simply from a study of the present day distribution of the world's flora, is more difficult than he has realised. For when inquiring whether the observed frequency distributions are too regular and of too frequent recurrence to be explained on the theory of Natural Selection, or how far on a close study they always appear to support the more mechanical theory

* *Origin of Species*, 6th Edition, Chapter IV. p. 128.

† Age and Area: briefly, that dispersal depends primarily on age and not on the special adaptation of character to environment.

Size and Space: briefly, that as mutations occur more or less mechanically, the older a genus the more species it will contain, and as age goes with area, so size (or number of species in genus) goes with space (or area).

proposed by Dr Willis, we may find it almost impossible to determine the magnitude of the correlation between age and area that is consistent with the one theory, or the point at which the "modifying" causes become so large that age can no longer be called the fundamental factor in the other.

(2) One of the first points that is brought out by an examination of the distribution of species in Ceylon and New Zealand is that the average dispersal of a wide species is considerably greater than that of an endemic species. In Ceylon the range has been determined by the six degrees of commonness (viz. V.C. = very common; C. = common; R.C. = rather common; R.R. = rather rare; R. = rare; V.R. = very rare) assigned to each species by Trimen in his *Handbook of the Flora of Ceylon**; and in New Zealand by the distance between the extreme northern and southern limits of the species' range (measured parallel to the length of the islands), estimated from the localities given in Cheeseman's *Manual of the New Zealand Flora*. Dr Willis's figures for the two floras are given in Table I† and show clearly the steady rise to a maximum in the sequences for each group

TABLE I.

(*a*) *Ceylon; Areal Classes.*

	V.C.	C.	R.C.	R.R.	R.	V.R.	Totals
Species endemic to Ceylon	19	90	139	136	192	233	809
Species found in Ceylon and Peninsular India only	45	118	103	84	64	78	492
Species of wider dispersal	221	462	313	209	159	144	1508

(*b*) *New Zealand; Longitudinal Ranges* (in miles).

	1080—881	880—641	640—401	400—161	160—1	Totals
Endemic Species ...	112	120	184	190	296	902
Wide Species ...	201	77	53	38	30	399

of endemics and wides. If the frequencies for individual families be taken‡, the same tendency is found, namely for the modal class for endemics to lie at the rare end of the scale and for wides at the common end. There is however considerable irregularity and Dr Willis was perhaps hardly justified in laying stress on the "wonderful likeness among the different families in the proportions of endemics

* Although the degree of commonness *within* the range as well as the extent of the limits of dispersal may have influenced the classifier, Dr Willis has found (*Annals of Botany*, Vol. xxxi. p. 190) that Trimen's classes give on the whole a fair measure of dispersal.

† For Ceylon, from *Phil. Trans.* Vol. 206 B. p. 310, Table III; for New Zealand, *Annals of Botany*, Vol. xxx. p. 449, Table VIII.

‡ For Ceylon, see for example *Annals of Botany*, Vol. xxx. pp. 7 et seq., Tables III, VI, VII and VIII; for New Zealand, *ibid.* p. 451, Tables XI and XII.

in the classes C., R.C., etc."* Dr Willis has argued that this distinction between the endemic and wide distributions cannot be explained on the theory of Natural Selection, yet it is interesting to find Darwin in a letter to Hooker† giving reasons why he would expect "that in islands a large proportion of the rarer forms would have been insular in their origin."

It has been pointed out that in taking averages the age factor is likely to be emphasised, so that if a large number of the endemic species are of comparatively recent origin—and Natural Selection suggests that they will be—it is not surprising to find that they cover on the average smaller areas than the wide species with continental range. We may examine this question from a slightly different point of view. It seems reasonable to suppose that at the period of the main plant invasions, the regions which are now Ceylon and New Zealand were part of much larger land masses out of which the present islands have been cut by submergence. If this has been so, we have not to think of the species of wide distribution entering across a comparatively narrow land bridge, but of a gradual invasion over a very broad front. In this way the geographical and even climatic boundaries, which in a limited space would have hindered or entirely checked the advance of a species, may have been taken in the flank or in the rear‡. But when, after submergence, the islands assumed their present shapes, the process of dispersal among new forms locally evolved will have occurred under more restricted conditions, so that the wide continental species will have had an initial advantage over the younger endemics which may still show in the tabulation of present day average ranges. The same problem arises in dealing with the distribution of species in the islands surrounding New Zealand; the present distribution must depend to a very great extent upon the past changes in the level of the land, and here our knowledge is only too scanty. Yet I believe it will be found that a little reasoning on the most general lines will lead us to many of Dr Willis's "predictions"§ without any need to minimise the importance of the part played by selection in determining individual dispersal.

But there is another point which touches very closely on the system of classification. In all countries a large number of "varieties" are found, both of wide and endemic species, which are usually restricted in range. The difference in character between the variety and the species will sometimes be so small that the two are barely distinguishable, and at other times so great that the systematist

* *Annals of Botany*, Vol. xxx. p. 7. To obtain a measure of the "likeness" of these series of frequencies, we may use the χ^2 Test for Goodness of Fit (*Biometrika*, Vol. viii. p. 250) to inquire whether the proportions for the individual families differ from those for the total endemic flora to a greater extent than we should expect in random sampling. It is found that, for Orchidaceae, $P = \cdot 04$; Rubiaceae, $P = \cdot 01$; Dipterocarpaceae, $P = \cdot 06$; and Euphorbiaceae, $P = \cdot 41$. That is to say, the proportions in four of the largest Ceylon families do not follow closely some general law.

† *More Letters of Charles Darwin*, Vol. i. Letter 365.

‡ In considering the plant invasions of New Zealand, Dr Willis discusses the outward spread of invading species along a comparatively narrow rectangular strip of land, but had the configuration of the coast followed at that epoch the present 1000 fathom line, for example, it is clear that the problem of dispersal would have been very different.

§ *Age and Area*, Chapter vii.

is uncertain whether to class the variety as a distinct species. We may suppose, for example, a genus containing a wide species (*A*) and an endemic species (*B*) both of which in the Ceylon classification are R.C., and that each has allied to it a well-marked endemic variety (*a* and *b*) of small range, V.R. Now had the differentiation between species and variety proceeded a little further, or were the classification to be undertaken by a more rigorous systematist, we should have had, instead of a genus of two species, namely:

(*A*) 1 wide, R.C. (*B*) 1 endemic, R.C.;

a genus of four species

(*A*) 1 wide, R.C. (*B*) 1 endemic, R.C.
(*a*) and (*b*) 2 endemics, V.R.

As the areal classification is by limits of range and not by area occupied, the change of the varieties (*a*) and (*b*) into species will not in general affect the class of the species (*A*) and (*B*). Not only therefore would a process of gradual divergence of varieties from their most nearly allied forms ensure a continual supply of small-range endemic species, but any tendency on the part of the botanist to subdivide species would lead to the same result. It will be found, in Ceylon, that the average proportion of rare endemic species in the genus increases with the number of endemics which the genus contains*; this is both in agreement with the point made by Darwin†, that there are on the average more varieties per species in large genera than in small; and with the suggestion that the size of certain large genera may have been created somewhat artificially by subdivision.

The more restricted dispersal of endemic species provides, in fact, no argument against Natural Selection; the question at issue is one of degree. Do the distributions typified in Table I recur within the separate families and larger genera with such regularity that a more mechanical rule of evolution is required? Individual opinion will differ, but the problem can be solved with no sweeping *a priori* argument, for smooth distributions occur again and again in statistics where no simple law of cause and effect is to be found.

(3) The Age and Area theory pictures a wide species, after entering a new country, gradually spreading out over an area of increasing size, and as it spreads "throwing" at intervals by mutation new endemic forms, which in turn spread out from their centres of origin. This regular process of dispersal and repro-

* I find the figures among 777 genera containing endemics, taken from Trimen's *Flora*, to be as follows:

	Class of endemics		
	V.C. and C.	R.C. and R.R.	R. and V.R.
Where 1 endemic in genus	30	68	72
,, 2 to 4 endemics in genus	35	84	118
,, 5 or more endemics in genus	40	114	216

† *Origin of Species*, 6th Edition, Chapter II. pp. 68—73. There are also several interesting letters of Darwin to Hooker and Asa Gray dealing with this subject and discussing the possibility of some unconscious tendency of systematists towards bias in recording varieties and species. (*Life and Letters of Charles Darwin* and *More Letters of Charles Darwin.*)

duction is obscured by the numerous modifying causes of environment, but if the theory be correct, it should still be possible to trace some form of relation within the genus between the parent wides and the endemics they have thrown. I have given in Table II the Ceylon species of the families of *Labiatae* and *Araceae*, with Trimen's areal classes; also the New Zealand species of *Umbelliferae*, classed according to longitudinal range from the information given in Cheeseman's *Flora**. It will be seen from the distributions of total frequency given at the bottom of the table that although the figures for *Labiatae* are irregular, with the modal class for endemics at C., and a secondary maximum for wides at V.R., those for *Araceae* and *Umbelliferae* are of the normal type of Table I. Yet if we examine the individual genera, we search in vain for any simple explanatory rule which will account for the relations of wides to endemics; there seems to be no evidence to show that the more common and therefore (on Age and Area) older wides have produced either more endemics or a greater proportion of common endemics. In the *Labiatae* we find that while in genus 1 (*Ocimum*), a V.C. indigenous wide (as well as two V.C.'s introduced species) and a V.R. wide have produced no endemics, two R.C.'s, a R.R., and a V.R. wide have produced a V.C. and C. endemic in genus 5 (*Plectranthus*); again in genus 8 (*Pogostemon*) there are no parent wides but a V.C., two C.'s and a R.R. endemic have appeared, while the three well-dispersed wides of genus 14 (*Leucas*) have produced no endemics. In the *Umbelliferae* we may ask why the five wides of *Hydrocotyle* after "throwing" four endemics which have had time to spread so as to cover as great a range as their parents, have "thrown" no recent species within the genus; while the single wides of *Eryngium, Actinotus, Apium, Oreomyrrhis, Crantzia* and *Daucus*, which have had time to spread over ranges from 400—1000 miles, have "thrown" no endemics unless into another genus; and why on the other hand in *Azorella, Aciphylla* and *Ligusticum*, where there are no wide species, are there numerous short range endemics? These particular areal classifications may be found to require modification, but this is not an isolated example; the same exceptions to the Age and Area rule can be found again and again in passing through the lists of genera. It can be argued that all these little inconsistencies arise from differences in habit, from the many environmental factors or are even the "usual variations due to the operations of the laws of trial and error," yet surely there is some danger that the fundamental principle will be reduced to insignificance by the innumerable modifying causes? These individual differences which Age and Area averages out, the theory of Natural Selection endeavoured to explain.

(4) The difficulty in applying any form of analysis to data such as those in Table II, is that the Age and Area theory cannot tell us which is the parent wide of each endemic. But suppose we confine our attention to those genera which contain one wide species only; here, on the theory, the endemics should have been

* In these and other distributions given in this paper I have omitted species which Trimen and Cheeseman considered to be probably introductions, and also in New Zealand I have followed Dr Willis in classing as wides, species only found outside New Zealand in one or more of the outlying islands, Kermadecs, Chathams, Snares, Aucklands, Campbells, Antipodes, Macquarie.

TABLE II.

Labiatae (Ceylon)			Araceae† (Ceylon)			Umbelliferae (New Zealand)							
Genus	Wide Species	Endemic Species	Genus	Wide Species	Endemic Species	Genus	Wide or Endemic	Range in miles					
								900—1080	720—900	540—720	360—540	180—360	0—180
1. Ocimum*	V.C., V.R.	—	1. Pistia ...	C.	—	1. Hydrocotyle	W.	3	1	1	—	—	—
2. Geniosporum	C., R.R.	—	2. Cryptocoryne ...	V.R.	R., V.R., V.R., V.R.		E.	3	1	—	—	—	—
3. Moschosma	R.R.	—	3. Lagenandra ...	V.C.	C., R.R., R., V.R.	2. Azorella ...	E.	—	1	1	—	2	3
4. Orthosiphon	R.C.	—	4. Arisaema ...	C., R.C.	V.R.	3. Eryngium ...	W.	—	—	1	—	—	—
5. Plectranthus	R.C., R.C., R.R., V.R.	V.C., C.	5. Typhonium ...	C., R., V.R.	—	4. Actinotus ...	W.	—	—	—	1	—	—
6. Coleus	C., R.C.	C., V.R.	6. Theriophonum	—	R.C.	5. Apium ...	W.	1	—	—	—	—	—
7. Anisochilus	R., V.R.	R.R.	7. Amorphophallus	C., R.	—	6. Oreomyrrhis	W.	—	1	—	—	—	—
8. Pogostemon	—	V.C., C., C., R.R.	8. Synantherias ...	R.C.	—	7. Crantzia ...	W.	1	—	—	—	—	—
9. Dysophylla	C., R.R.	—	9. Remusatia ...	R.C.	—	8. Aciphylla ...	E.	—	2	—	1	2	6
10. Mentha	R.R.	—	10. Colocasia ...	V.C.	—	9. Ligusticum ...	E.	—	1	—	3	2	8
11. Calamintha	C.	—	11. Raphidophora...	V.C., R.R.	—	10. Angelica ...	E.	—	1	2	1	—	1
12. Scutellaria	C. (?), V.R.	C.	12. Lasia ...	R.C.	—	11. Daucus ...	W.	1	—	—	—	—	—
13. Anisomeles	C., R.C.	—	13. Pothos ...	V.C.	R.C.								
14. Leucas	V.C., V.C., C., R. (?) V.R., V.R. (?)	—	—	—	—								
15. Leonotis	C.	—	—	—	—								
16. Teucrium	R.C.	—	—	—	—								

Total Distributions :

		V.C.	C.	R.C.	R.R.	R.	V.R.	Totals
Labiatae	Wides	3	8	6	5	2	6	30
	Endemics	2	5	—	2	—	1	10
Araceae	Wides	4	4	4	1	2	2	17
	Endemics	—	1	2	1	2	5	11

Total Distribution :

	900—1080	720—900	540—720	360—540	180—360	0—180
Wides ...	6	2	2	1	—	—
Endemics ...	3	6	3	5	6	18

* Of the four species of Ocimum entered in the catalogue, two V.C.'s are probably naturalised and have therefore been omitted.

† The two species of the genus Alocasia and the single species of Acorus have been omitted as probably introduced.

"thrown" in the great majority of cases either by this wide or by other endemics previously "thrown" by it. We should therefore expect to find:

(*a*) The greater the range of the wide, the larger will be the number of endemics in the genus.

(*b*) The range of the wide and the range of the most common endemic in the genus should both be functions of the time which has elapsed since the wide was first introduced into the country; hence there should be a positive correlation between them.

The following data have been collected from the Ceylon Flora and the New Zealand Flora to test these points.

Ceylon.

(*a*) The frequencies required are given in Table III. We see first that the wide species in genera containing no endemics are of somewhat smaller range than those in genera with endemics; this is brought out more clearly by percentage figures:

	Genera with no endemics	Genera with endemics
Percentage of V.C. species	9·9	14·1
" " species C. or more common	40·2	48·7
" " " R.C. " "	63·7	69·2
" " " R.R. " "	80·9	84·6
" " " R. " "	90·3	98·7

But when we examine the bottom row of figures in Table III we see that there is no increase in the number of endemics associated with the wide as we pass from R. to V.C. wides. It is true that in only one genus out of 43 are there endemics with a V.R. wide, but for the other classes we find the V.C. wides actually with the smallest and the R. wides with the largest average number of endemics, although the differences can hardly be considered significant.

TABLE III.

Ceylon; Distribution in Genera containing only one Wide Species.

	Class of Wide Species.						
	V.C.	C.	R.C.	R.R.	R.	V.R.	Totals
Genera containing no endemics ...	43	132	102	75	41	42	435
Genera containing one or more endemics	11	27	16	12	11	1	78
Totals ...	54	159	118	87	52	43	513
Total number of endemics associated in genus with different classes of wide	15	50	36	28	17	4	150
Average number of endemics in genus ...	·278	·314	·305	·322	·327	·093	

(*b*) Table IV gives the distribution of the 78 genera containing one wide species, classified according to the ranges of the wide and of the most common endemic; the cell frequencies are of course very small. In the genera with R. wides, the endemics are nearly all R. or V.R. but beyond this there is no marked

TABLE IV.

Ceylon ; Genera containing only one Wide Species.

Class of commonest Endemic in Genus.	Class of Wide. V.C.	C.	R.C.	R.R.	R.	V.R.	Totals
V.C.	1	1	—	1	1	—	4
C.	2	7	4	1	—	—	14
R.C.	1	7	4	3	1	—	16
R.R.	3	6	4	4	—	—	17
R.	1	2	4	1	5	1	14
V.R.	3	4	—	2	4	—	13
Totals	11	27	16	12	11	1	78

clustering of frequencies about the diagonal. Combining the R. and V.R. wides and the V.C. and C. endemics so as to form a table of 25 cells, the mean square contingency, ϕ^2, has been calculated. It is found that

$$\phi^2 = \cdot 2988,$$

but with so small a sample and so many cells, the mean value of ϕ^2 for zero contingency is large*, namely

$$\bar{\phi}_0^2 = \cdot 3077 \pm \cdot 0611,$$

so that the value of ϕ^2 cannot be considered significant.

If the frequencies are combined to form a four-fold table by putting together the V.C., C. and R.C. species in one group and the R.R., R. and V.R. in another, the resulting value of the tetrachoric coefficient of correlation is

$$r = + \cdot 321 \pm \cdot 117,$$

which is probably significant.

In order to obtain a larger population for the contingency table we may even extend the method to all genera, and inquire whether there is any relation between the range of the most common wide in a genus and that of the most common endemic†. Using 300 Ceylon genera containing endemics, in a contingency table of 20 cells, it was found that

$$\phi^2 = \cdot 0328,$$

while

$$\bar{\phi}_0^2 = \cdot 0633 \pm \cdot 0142,$$

so that the mean square contingency is again insignificant.

* *Biometrika*, Vol. XI. pp. 215—230. $\bar{\phi}_0^2 = \frac{c-1}{N} \pm \cdot 67449 \frac{\sqrt{2c}}{N}$ approximately, where c is the number of cells and N the size of sample.

† The range of the most common wide and also of the most common endemic in a genus should both, on the Age and Area theory, provide some measure of the time which has elapsed since the genus or its first wide species entered the country.

New Zealand.

(*a*) Of 133 genera in Cheeseman's *Flora* containing one wide species, I find 41 containing endemics. Taking the longitudinal range in the Islands as a measure of dispersal, the mean range for these wide species is:

(i) In genera containing no endemics, $731 \pm 22{\cdot}5$ miles.

(ii) In genera containing endemics, $814 \pm 36{\cdot}0$ miles.

If, however, the range of the wide and the number of endemics in the genus are correlated, it is found that:

(i) For the whole 133 genera, $r = +{\cdot}0606 \pm {\cdot}0583$,

(ii) For the 41 genera containing endemics, $r = +{\cdot}0151 \pm {\cdot}1053$,

neither of which correlations is significant. We see therefore, as in Ceylon, that the dispersal of the wide is on the average slightly greater when there are endemics in the genus, but that beyond this the actual number of endemics—whether 1, 10 or 20—does not increase with the range of the wide.

(*b*) The relation between the dispersal of the wide and of the most common endemic in the genus was not examined for New Zealand.

The analysis of the present section (4) has not brought out any clear cut relationships of significance sufficient to assist in discriminating between one theory of evolution and another. If it be argued that in combining together the species from different families, the principles of Age and Area and of Size and Space have been obscured, it must be shown that relations between parent wide and offspring endemic do hold within the individual families or in what Dr Willis has termed groups of allied species; a study of the figures of Table II and similar distributions hardly suggests that this is the case. All the evidence in fact suggests that age must take its place with all the factors of past and present environment, as one of many causes influencing the development and dispersal of species.

(5) The most important evidence in support of his theory that Dr Willis has drawn from New Zealand is concerned with the regular sequences of zonal frequencies* found not only for the total flora, but for the species within individual families and genera. The total frequencies for the wides and endemics are given in Table V†; it will be seen that in both cases the figures fall off towards the ends of the island range, but that while the distribution for wides is almost the same in both Islands, there are far fewer endemics in the North than in the South Island. Dr Willis has not published any map, as far as I am aware, showing the position of his zonal boundaries, but as the length of the curve stretching from the North Cape to the south of Stewart Island, from which the transverse sectors were marked off at intervals of 100 miles, is given as 1080 miles, it has been possible to obtain approximately the positions of the bounding lines

* I.e. the number of species found within the zones or sections of New Zealand determined as described below.

† The frequencies are taken from *Annals of Botany*, Vol. xxxi. p. 201.

and the relative areas of the eleven zones. The former are shown in Figure 1, and the latter are given at the bottom of Table V*. It will be seen that the scarcity of endemics in zones I, II, III and XI is associated with very small zonal areas; if New Zealand is to be treated as a long curved strip of land 1080 miles long with an average breadth of 100 miles, it must not be forgotten how far below the average is the width of the three northern zones. If two zones contain the

TABLE V.

New Zealand Zonal Frequencies.

	North Island						South Island				Stewart Island
Zones (range in miles)	I 0—100	II 100—200	III 200—300	IV 300—400	V 400—500	VI 500—600	VII 600—700	VIII 700—800	IX 800—900	X 900—1000	XI 1000—1080
Wides ...	209	210	237	237	235	242	236	227	215	204	112
Endemics ...	234	280	330	368	386	537	532	527	516	414	130
Approx. area in 100 square miles	23·0	38·7	60·0	198·7	106·9	56·8	106·3	104·7	125·7	159·3	42·2

same number of wide species, but one is a quarter the area of the other, it is clear that the number of endemic species in the two may differ; for as has been pointed out above, it is probable that more new forms will be evolved in the larger area where the flora is subject to more varying conditions of environment and also probably consists of a larger number of individual plants.

The past land configuration is however more important than the present. Although there is no very certain information regarding the form of New Zealand in past geological times, the land appears to have been subject to several alternate periods of depression and elevation. Marshall† considers that in the middle of the Pleistocene period, after a period of elevation, New Zealand was united with the outlying islands and extended over the whole of the New Zealand plateau; that a great depression then followed which lowered the land below its present level, and that this has been followed by continuous elevation which locally at any rate is still in progress. A depression of the land to or below its present level,

* Taking a large map of New Zealand and using a spline, a curve of 1080 miles in length was drawn from the North Cape to the level of the south-west corner of Stewart Island as shown in Figure 1. The eleven zones were then marked off by straight lines drawn perpendicular to this central curve, and their areas measured with a planimeter. It appears from a footnote to p. 444, *Annals of Botany*, Vol. xxx. that Dr Willis's zone VI (range 500—600 miles) includes parts of both South and North Islands; I found 19·0 hundred square miles in the former and 37·8 in the latter. As Stewart Island does not stretch from 1000—1080 miles, I have included a part of the South Island (area 36·2 hundred square miles) in zone XI, but Dr Willis's XIth zone may contain only Stewart Island.

† P. Marshall, *New Zealand and Adjacent Islands*, in the series *Handbuch der Regionalen Geologie*, Heidelberg, 1911.

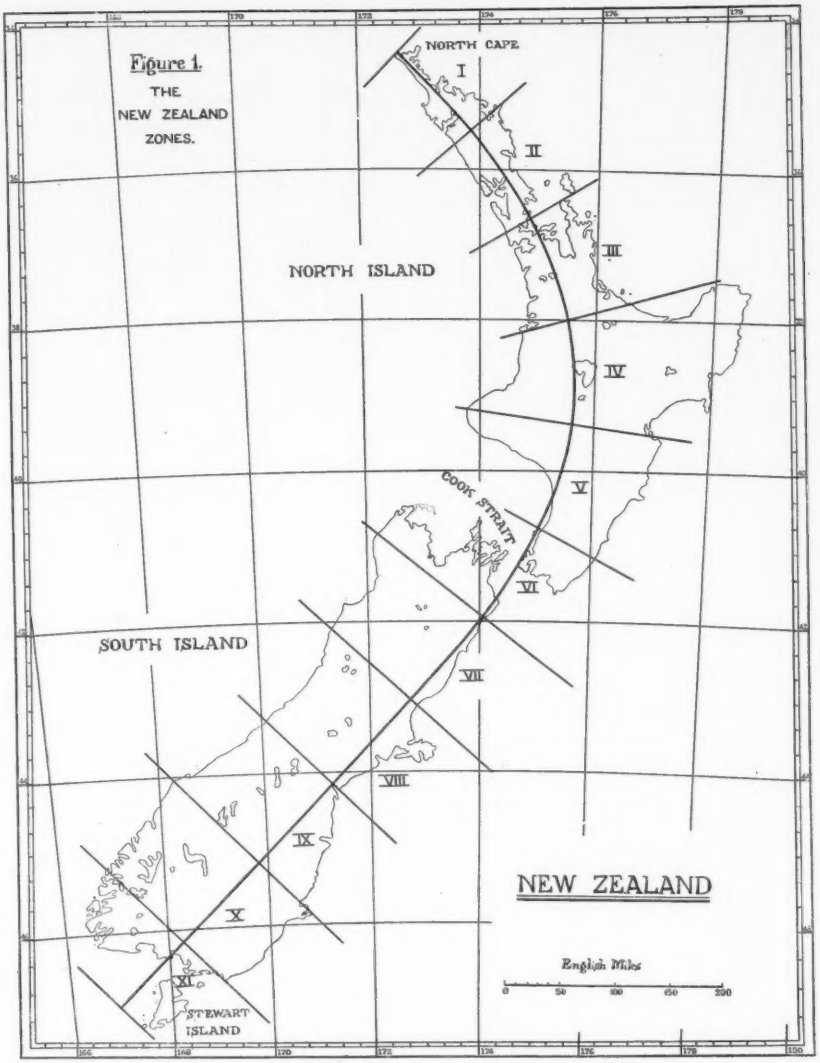


Figure 1.
THE NEW ZEALAND ZONES.

after the species of wide distribution had become fairly uniformly spread over it* would not have affected the relative frequencies of wides in different areas which remained above the sea, but had the submergence of the northern section been greater than that of the southern, the number of new forms evolved in the more

* That is to say if there were on the whole the same *number* of species, though not of course the same species, in each 1000 or each 10,000 square miles.

restricted areas of the north might perhaps have been smaller than in the south. I do not however wish to lay stress on any particular explanation, but rather to point out how largely the present distribution of species may have been influenced by past changes in the level of the land about which we know only too little.

But apart from this difference in size between the North and South Island figures for endemics, it is still necessary to explain the regular falling off in the zonal frequencies, towards the outer extremities of the islands, which occurs again and again in the individual families and genera. Dr Willis believes it to be due to the outward spread of the majority of the wides from one or more central points or belts of invasion, accompanied by a rather mechanical process of endemic "throwing." His argument is developed fully in the first New Zealand paper*. But is this the inevitable conclusion to be drawn from a study of series after series of figures that he gives in his Tables† with their remarkably regular rise and fall? It appears to me that had the majority of species spread outwards to their present limits of dispersal from smaller areas in which they had either originated as new forms or had been confined during a period of greater land depression, there might well be a noticeable shortage of species at the ends of this long and narrow range of islands due to the fact that while in a central section species would enter from both sides, in an end section there would be no spreading in from the sea. This "end" effect may be illustrated experimentally in the following manner. Suppose the line AB in Figure 2 to represent the length of New

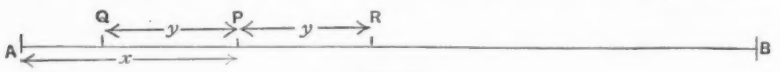


Figure 2.

Zealand, and that QR is the longitudinal range of a species which has spread out to distances y on either side of a centre P, where $AP = x$. Then to obtain a general random distribution for N species we must arrange that

(a) the N centres, P, are randomly distributed along AB,

(b) the frequency of ranges, y, obeys some particular law, $z = f(y)$,

(c) y is uncorrelated with x.

If we are considering the distribution of endemics, the law $z = f(y)$ must be of the same form as that observed in New Zealand and Ceylon, that is to say it must give many species covering only small areas and few covering large areas; this does not of course require that *age* should have been the chief factor determining the value of y for a particular species. Taking the unit for y as one-tenth of the length AB, the frequency distribution chosen for 100 species was as follows:

z, number of species	25	12	9	8	7	6	5				
y, range of dispersal	0·5	1·0	1·5	2·0	2·5	3·0	3·5				
z	5	4	4	3	3	2	2	2	1	1	1
y	4·0	4·5	5·0	5·5	6·0	6·5	7·0	7·5	8·0	8·5	9·0

* *Annals of Botany*, Vol. xxx. pp. 442—444. † *Ibid.* Vol. xxx. pp. 444—447, Tables III—VI.

This distribution gives a steady fall in frequency from 25 species whose range ($2y$) is one unit, to one species whose range would be 18 units were the island long enough. There is no special merit in the exact figures chosen; they were fixed arbitrarily to make the results agree approximately with the New Zealand distribution. The requirements (a) and (c) were then fulfilled by a process of random sampling*, and the resulting distribution is illustrated in Figure 3, where the black circles represent the centres, P, of Figure 2, and the lines stretching out on either side, the range QR. The distribution of ranges in Figure 3 is given in Table VI; owing to the limits formed by the ends of the island, the frequencies will not of course correspond to the z-frequencies given above. They should be compared with the actual New Zealand frequencies given by Dr Willis† and repeated at the bottom of Table VI. There are clearly too many species in the theoretical distribution with range 9·1—10·0, but this is simply because too many species with large values of y were taken in the arbitrarily chosen z distribution; this could no doubt be altered by a little readjustment, but no exact conformity has been aimed at.

TABLE VI.

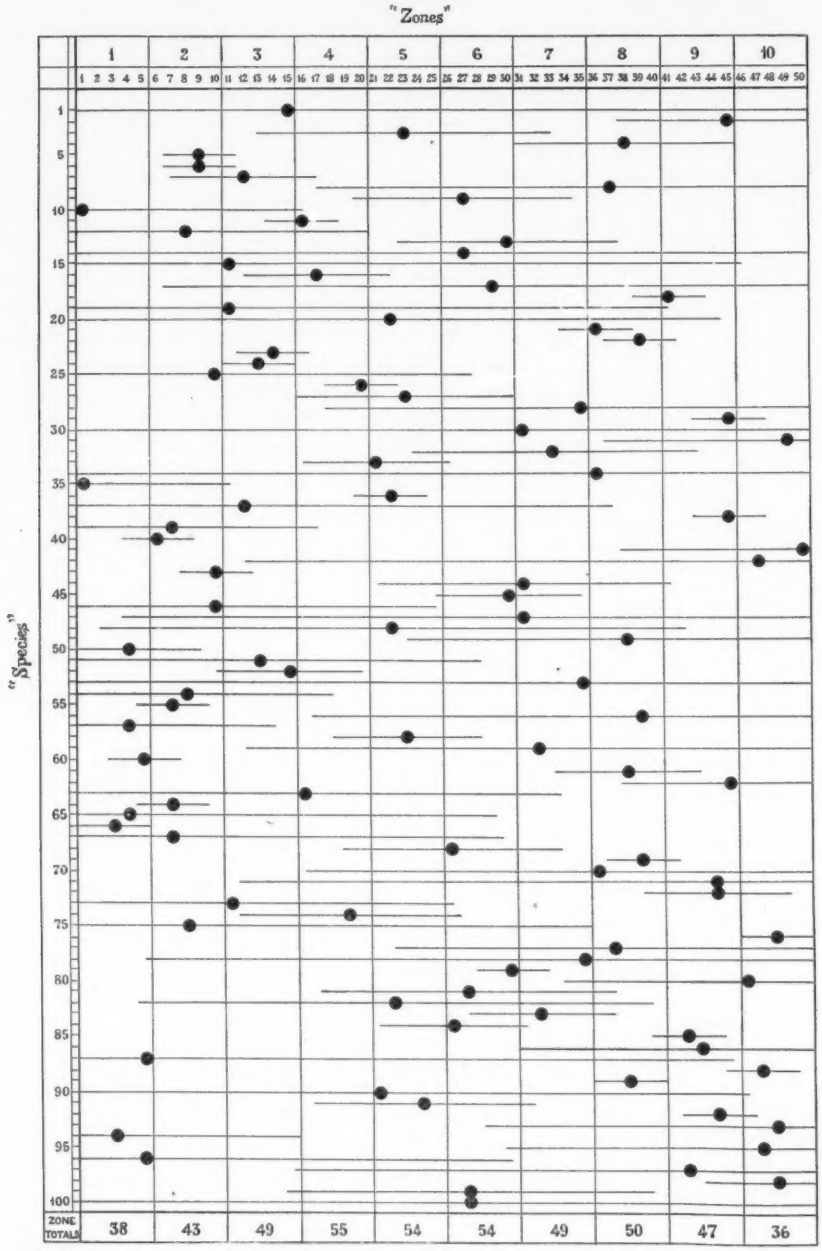
Theoretical distribution of 100 species	Range in zone units	0–1·0	1·1–2·0	2·1–3·0	3·1–4·0	4·1–5·0	5·1–6·0	6·1–7·0	7·1–8·0	8·1–9·0	9·1–10·0
	Frequency	25	12	14	10	4	8	7	6	4	10
Observed distribution of all endemics	Range in miles	1–40	41–160	161–280	281–400	401–520	521–640	641–760	761–880	881–1000	1001–1080
	Frequency	168	128	93	97	105	79	61	59	60	52

The diagram of Figure 3 has been divided into 10 zones by transverse lines at equal intervals; the zonal frequencies are given at the bottom of the figure and show just the same type of rise and fall which would appear in the figures for a large New Zealand family such as the Scrophularineae, if the North Island frequencies were as high as those in the South Island. The following figures for

* The procedure adopted was as follows: 50 divisions were marked and numbered on the line AB at equal intervals. 300 slips of paper, 6 representing each division, were shuffled in a box; 100 slips were then drawn out at random and the corresponding division-numbers written down in the order of drawing; this secured a fairly uniform but random distribution of the centres, P. Then another 100 slips, 25 of which were marked ($y=$) 0·5, 12 marked ($y=$) 1·0, etc....., were shuffled and drawn in order, the first drawn being placed against the first division-number drawn, the second against the second division-number drawn, and so on through the 100. The order of drawing is the order from top to bottom of Figure 3.

† *Annals of Botany*, Vol. xxx. p. 449, Table VIII. The range for Dr Willis's first group is only 40 miles and in the last group only 80 miles, while in the eight middle groups it is 120 miles. This must be remembered in comparing the two sets of figures.

Experimental Random Distribution of 100 *"species" in ten zones of a narrow "Island."*



The black circles represent the random centres of origin of the 100 "species," and the horizontal lines the random "ranges" over which each "species" is spread.

Figure 3.

the ten zonal frequencies obtained from a completely independent sampling show that the distribution of Figure 3 is not exceptional*:

46, 53, 57, 56, 51, 53, 47, 39, 39, 30.

The order in which the theoretical species are entered in Figure 3 is purely random, and if we take from this smaller groups of consecutively numbered species to represent genera, we shall have in each genus so selected

(*a*) a random distribution of centres, *P*,

(*b*) a distribution of ranges selected randomly from the frequencies of Table VI.

The zonal frequencies for such theoretical genera may be compared with the actual frequencies for New Zealand genera tabled by Dr Willis†. I have therefore given in Table VII groups of consecutive species chosen arbitrarily out of the 100

TABLE VII.

Theoretical Zonal Distribution of Species.

No. of Species in Genus	Species Numbers from Figure 3	"Zones." 1	2	3	4	5	6	7	8	9	10
25	1—25 (inclusive)	8	12	17	16	12	11	12	13	11	6
	46—70 "	15	15	12	15	14	13	10	10	10	7
	71—95 "	7	7	9	10	12	16	16	15	13	12
10	19—28 "	3	3	5	7	6	5	4	5	4	1
	34—43 "	5	6	6	5	4	3	3	4	4	4
	61—70 "	5	4	3	5	5	5	4	4	4	2
	78—87 "	3	3	3	4	5	7	9	8	5	3
5	3—7 "	—	3	4	2	1	1	2	1	1	—
	13—17 "	2	3	4	4	5	4	4	4	3	3
	26—30 "	1	1	1	4	4	3	2	2	3	3
	50—54 "	4	5	4	4	2	2	1	1	1	1
	68—72 "	—	—	1	3	3	3	3	4	4	3
	89—93 "	1	1	1	2	2	3	3	3	3	3
3	1—3 "	1	1	2	2	2	2	2	2	2	2
	27—29 "	—	—	—	2	2	2	1	1	2	2
	58—60 "	1	1	1	2	2	2	1	1	1	1
	83—85 "	—	—	—	—	1	2	2	2	1	—
	Total for "Family"	56	65	73	87	82	84	79	80	72	53

* This distribution is noticeably skew; as it was possible that this arose because certain of the slips of paper used in the process of sampling were larger than others, a fresh set of slips was prepared for the sampling which resulted in Figure 3. It is possible that with an increased bias of this sort, which might correspond to some natural conditions, the observed unequal distributions of the North and South Islands might be obtained approximately.

† *Annals of Botany*, Vol. xxx. pp. 444—447, Tables IV—VI.

species to represent 3 genera of 25, 4 genera of 10, 6 genera of 5 and 4 genera of 3 species*. It will be found that the theoretical distributions are not quite as smooth as those given in Dr Willis's tables, but the general trend is almost invariably a rise from one or both sides to a single maximum. Yet this lack of smoothness may well be explained, if it is remembered that in the model, species of each genus, however small, have been centred at points scattered absolutely at random over the island, while in the natural case there must be a certain degree of continuity of range among the endemic species of the same genus which may be not only of common origin but dispersed within the same geographical and climatic limits from a similarity of habit.

If the North and South Island distributions are considered separately we might expect to find a slight fall in zonal frequency towards the south of the former and the north of the latter island resulting from this "end" effect, although the sea barrier at Cook's Strait has been more recent and less effective than at the outer ends of the islands. It is not possible to test this point from Dr Willis's published frequencies, as species from both islands are grouped together in his middle zone, VI, but by identifying Cheeseman's localities for four families I have obtained the following frequencies for ten fresh zones, the division between the Vth and VIth zone lying at Cook's Strait.

TABLE VIII.

	North Island					South and Stewart Islands				
Range in miles	0—110	110—220	220—330	330—440	440—550	550—660	660—770	770—880	880—990	990—1080
Umbelliferae ...	4	5	10	12	13	25	27	25	29	21
Leguminosae ...	2	2	5	8	6	13	17	14	15	5
Rubiaceae ...	17	16	24	25	23	24	24	20	19	17
Scrophularineae (Veronica only)	6	7	8	17	16	43	46	42	38	16
Totals ...	29	30	47	62	58	105	114	101	101	59

As well as the marked discontinuity between the North and South Island frequencies, these figures do show a slight fall at the inner ends of the islands (zones 440—550 and 550—660 miles), but the distribution in more families is required to judge the significance of this drop.

Without claiming any exact correspondence with the actual phenomena, the illustration which has been given shows the importance of the "end" effect in a restricted distribution. The increase in the frequencies of the central zones over

* By this method the same "species" has been included in more than one "genus," but this does not matter for the present purpose of illustration.

those of the outer zones appears to be the almost inevitable result of any tendency in the past for greater dispersal among species whose range fell well short of the length of the islands, and particularly in those genera where new forms have been evolved since the country roughly assumed its present configuration. In showing how a random distribution might account for the observed frequencies we have provided no definite evidence against Age and Area, but it would seem that the existence of these frequencies cannot be used with any justification as a decisive argument in favour of the theory*.

(6) Developing his theory of the zonal frequencies, Dr Willis attempted,

(*a*) to divide the wide species into three or four large groups which invaded New Zealand at different times and at different points, and to assign to these groups the endemics "thrown" by the wides;

(*b*) to argue that the more recent origin of the endemics is confirmed by the fact that the zonal frequencies for endemics fall off more rapidly from the maximum frequency (whose position corresponds roughly to the point of invasion) than for wides, because the former have not had time to spread so far as the latter.

Northern Invasion.

In a certain number of groups of species from about 35 families there is a marked tendency for the zonal frequencies to have a maximum in the north, in zone I or II, and for the figures to fall off steadily towards the south. Great caution is necessary in picking out genera in this way, since it will be found that about one-third of the experimental random distributions of Table VII show a maximum in zones 1 or 2, or in 10 or 9, but Dr Willis believes that he has been able to connect these 99 endemic and 31 wide species by the link of an Indo-Malayan origin†. The total frequencies, 90 and 635, are distributed as follows:

Zones	I	II	III	IV	V	VI	VII	VIII	IX	X	XI
Wides	13	11	12	10	8	8	7	7	6	5	3
Endemics	84	89	87	78	64	62	50	40	35	32	14

To test the significance of the difference between these two sequences, the χ^2 Test for Goodness of Fit may be applied; I find that $P = \cdot 998$, so that the odds are enormously in favour of the two series being random samples of the same population. That is to say, no argument as in (*b*) above can be based on the slightly smaller proportions of endemics found in the four southern zones‡.

Southern Invasions.

It has been pointed out in the preceding section that there is no reason to suppose that the observed central maxima in the endemic distributions correspond

* For example in *Annals of Botany*, Vol. XXXII. p. 342, after referring to "such extraordinary results as those given in Tables IV—VI" of the first New Zealand paper, Dr Willis writes that "it is perfectly obvious that such results must be explained, and it is almost equally obvious that no explanation other than that they are the result of a mechanical cause will meet the necessities of the case."

† *Annals of Botany*, Vol. XXXII. pp. 355—358.

‡ *Ibid.* p. 357 and again in his book *Age and Area*, pp. 79—81, Dr Willis has given curves representing the zonal frequencies and has argued that the two distributions are different because the *slope of the endemic curve* (which contains the larger population) *is steeper*.

to the positions of points or belts of invasion. The distribution for the 803 endemic and 368 wide species not attributed to the northern invasion is:

Zones	I	II	III	IV	V	VI	VII	VIII	IX	X	XI
Wides	196	199	225	227	227	234	229	220	209	199	109
Endemics	150	191	243	290	322	475	482	487	481	382	116

The figures show:

(*a*) The wide and endemic proportionate frequencies for the five South Island zones VI—X* do not differ significantly; applying the χ^2 Test, I find a value for P of ·58, or random sampling would more often than not give differences as great as those observed.

(*b*) A sudden discontinuity occurs in the endemic frequencies between zones V and VI which does not appear in the northern invasion frequencies; this marks the position of Cook's Strait, which lies in zone VI.

(*c*) The proportion of wides to endemics in the three very small northern zones I—III is roughly the same as that in the small southern zone XI†; it seems likely that restriction of range has played an important part in determining distribution in both cases.

These facts have undoubtedly to be interpreted in terms of the geological history and the plant invasions of the past, but once it is allowed that smooth frequency distributions can be explained without resort to a "mechanical" theory of evolution, it can hardly be claimed that the Age and Area hypothesis will provide the only solution.

(7) There are many interesting questions which arise out of the interpretation given by Dr Willis and Mr G. Udny Yule to the observed "hollow" curves which are followed by the frequency distributions for sizes of genera‡, but they must be left for discussion in a further paper; the object of the present paper has been to discuss the bearing of the principles of Age and Area, and Size and Space on the distribution of the floras of Ceylon and New Zealand. There is no doubt whatsoever that these principles represent a certain aspect of the process of evolution, but I believe that Dr Willis has stressed their importance beyond the limits which the evidence of observation will bear. They cannot explain everything, and we have seen that in many cases results which we are led to predict with their assistance are scarcely borne out, while in other cases smooth recurrent distributions can also be accounted for on different hypotheses. Darwin's theory may not be adequate (that remains to be seen), but before it is declared to be obsolete, —"a limiting factor in the progress of biology,"—it is necessary that some more crucial tests should be produced to discriminate between it and the theory which is to replace it, than appear so far to have been provided in the statistical results brought forward in support of Age and Area.

* Zone VI of course includes a portion of the North Island.

† The approximate zonal areas were given in Table V.

‡ See *Nature*, Feb. 9, 1922, pp. 177—179, and the later chapters in *Age and Area*.

THE INTERRELATIONSHIPS OF THE PHYSICAL MEASUREMENTS AND THE VITAL CAPACITY.

By ALFRED A. MUMFORD and MATTHEW YOUNG.

Introduction by Dr Mumford.

The data, on the analysis of which the following paper is chiefly based, were gathered together at the ordinary routine physical measuring of all the boys at the Manchester Grammar School made during June and July 1921. No discrimination has been made between boys of poor physique and those who are robust. Regular bodily measurements of physical stature, weight, chest girth, forearm and upper arm were begun in 1881 and have been continued annually since in order to obtain a clear knowledge of the alteration of physique of each individual boy which accompanied his regular training in the gymnasium.

In 1921 it was decided to add measurements of vital capacity and trunk length, partly in order that the annual increase in vigour might be watched and partly to serve as a basis for inquiring into the relationships between respiratory range and bodily structure.

A few facts concerning the social status and intellectual quality of the group of boys here studied may be of value, particularly as the data for boys at the ages 16, 17 and 18 may possibly not be regarded as entirely comparable with those for younger boys. This arises from the fact that there is only one set of measurements for each boy and that a more or less selective change takes place at the higher ages since many of the less studious and many of the precociously physically robust boys leave early, while, on those who remain, the depressing influence of concentrated study may exert a more marked effect than in a school where physical development in the higher forms is not interfered with by examinations.

The School provides an intensive curriculum for 1200 boys of whom about 15 per cent. are of Jewish parentage. About 45 per cent. of the boys come from middle class preparatory or other secondary schools and 55 per cent. from public elementary schools. Of the latter about one-half are highly selected since they represent the upper 10 per cent. of those who have been chosen by the headmasters of local elementary schools to compete for and have obtained entrance scholarships or "free places." They generally come from homes which are making considerable sacrifices to obtain a better training for their children than the parents have themselves received. The boys are therefore well cared for and supervised. In physique, in scholarship, and in athletic energy and attainments they maintain their level among their fellows. Between 20 and 25 per cent. of those who leave the School pass on to a general University or to the local Technological College,

which is really a constituent part of the local University. Many are assisted by scholarships of various kinds. It may be said, therefore, that the majority of the boys here measured have been selected by innate powers, even more than by social circumstances, for higher education.

Analysis of Observations by Dr Young.

The relationships that exist between the several measurements of the human frame and between these and the vital capacity have long been a subject of interest. Various attempts have been made, since Hutchinson's pioneer work on the vital capacity, to construct formulae which would define in a simple form these relationships either as an aid to the diagnosis of disease or to the study of nutrition or for other purposes. Amongst those, with whose names such formulae are associated, are Pfaundler, Von Pirquet, Lissauer, Bornhardt, Ainley Walker and Dreyer. Professor Dreyer* has contributed largely to the subject. He has calculated formulae which are alleged to define in a simple manner the association between the several pairs of measurements of the human body and their individual relationship to vital capacity in his investigation into the latter as a criterion of physical fitness. He has also published an interesting series of tables, based on his formulae, showing the normal in one measurement for varying values of the others. These formulae were derived originally from a relatively small number of observations, though their suitability appeared to be confirmed by the results obtained in longer series of data, namely, Hutchinson's and Schuster's, to which they were applied. There still seems to be, however, very considerable doubt as to whether one formula can be applied to define, with a sufficient degree of accuracy for general and practical use, the relationships between any pair of physical measurements or between these and vital capacity throughout the whole course of life as claimed by Professor Dreyer. Further investigation in large series of cases seems necessary and desirable to substantiate or disprove such an assertion before his formulae and tables come into wider and more general use by other observers, or gain general acceptance as providing more than a rough method of approximation.

Material used. While, as has been mentioned, the data on which the present inquiry was made were chiefly obtained from the boys of Manchester Grammar School, frequent reference will also be made to results calculated from figures for adults published by Arnold† and figures for boys and adults kindly supplied for analysis by Dr Hobson.

Full details were available for about 1100 boys of the Grammar School of ages ranging from 11 to 19 years. In taking the measurements, the methods and instructions of Professor Dreyer were carefully followed in estimating the stem length, the chest circumference and the vital capacity. In regard to the weight,

* Dreyer, Georges (1920). *The Assessment of Physical Fitness.* Cassell and Co.

Idem. (1919). "Investigations on the normal vital capacity in man and its relation to the size of the body." *The Lancet*, August 9th.

† Arnold, Friedrich (1855). *Über die Athmungsgrösse des Menschen*, Heidelberg.

however, the method in use at Oxford was not accurately followed. At first it was decided to obtain the weights without clothes, but this method proved inconvenient and the weights were ultimately taken in the ordinary gymnasium costume. This has been found, after frequent estimates, to weigh very constantly between 4 and 5 per cent. of the body weight for all ages. While it is recognised that weights in the nude condition are more reliable and desirable for comparative purposes, the weights as they have been taken should not impair to any appreciable extent or render invalid the results that have been obtained or vitiate the conclusions that have been drawn from them.

The data that were utilised, of those available in the case of each boy, were the exact age in years and months, the standing height, the stem length or trunk length, the weight, the chest circumference and the vital capacity. While the interrelationships of these variables have been considered generally in the present investigation, special attention has been directed to the relationship shown between the two measurements of length—viz., full and stem,—and body weight and vital capacity respectively.

The mean values of the measurements under consideration with their standard deviations and coefficients of variation for the boys at the several ages and for all ages together are shown in Table I. One of the most striking features is the relatively wide range of variation that is met with in vital capacity and weight even in boys of the same age. The high degree of variation to which the latter measurement is subject has already been noted in other data by several observers and is now well known. The values of the coefficients of variation for full stature and stem length seem to indicate quite clearly that full stature is not more variable than stem length although this has been frequently suggested and has been used as an argument in favour of the use of the latter measurement in preference to the former.

Correlation shown in Data. It was decided to determine what relationships would be shown to exist between the several pairs of measurements in the data by the method of correlation as a preliminary to further investigation by Professor Dreyer's methods. The correlation coefficients were thus calculated between the several pairs of measurements for all the boys taken together, irrespective of age, and for the groups of boys at each age. These coefficients are shown in Table II, with the numbers at each age on which the calculations are based. The numbers at ages 11 and 18 are comparatively small and the coefficients at these ages, though calculated and inserted in the table, as they are less reliable than the others, have been disregarded in the formulation of any conclusions from the values obtained. As a general index of comparison of the degree of correlation between the several pairs of measurements, the mean values of the coefficients for the different ages from 12 to 17 should be taken rather than the coefficients for all the boys grouped together. The latter coefficients are appreciably higher than the mean values of the corresponding coefficients found for boys at the several ages which is probably largely due to the circumstance that each of the two variables is quite definitely

TABLE I.

Manchester Grammar School Data. Mean Values.

Age	Number of Observations	Standing Height			Stem Length			Vital Capacity			Weight			Chest Circumference		
		Mean cm.	S. D. cm.	C.	Mean cm.	S. D. cm.	C.	Mean c.c.	S. D. c.c.	C.	Mean kilog.	S. D. kilog.	C.	Mean cm.	S. D. cm.	C.
11 years	29	141·00	5·15	3·60	72·71	2·37	3·26	2155	266·40	12·36	33·12	3·47	10·48	65·66	2·98	4·54
12 „	112	145·86	8·03	5·51	74·45	3·83	5·14	2420	397·20	16·41	35·55	5·24	14·74	66·31	3·80	5·73
13 „	173	150·60	7·98	5·30	76·58	3·65	4·77	2578	465·90	18·07	40·14	6·30	15·70	69·44	4·08	5·88
14 „	255	156·97	8·97	5·71	79·81	4·47	5·60	2857	513·30	17·97	44·91	7·45	16·59	72·49	4·87	6·72
15 „	263	164·08	8·46	5·16	83·59	4·41	5·28	3268	573·00	17·53	50·30	7·42	14·75	76·24	5·23	6·86
16 „	173	167·81	8·31	4·95	85·79	4·54	5·29	3506	627·50	17·90	54·76	8·01	14·63	78·74	5·09	6·46
17 „	80	170·42	6·29	3·69	88·00	3·56	4·05	3713	678·30	18·27	57·30	7·22	12·60	81·01	4·63	5·72
18 „	25	171·66	7·66	4·46	89·00	4·08	4·56	4207	674·00	16·02	60·42	7·04	11·65	82·62	4·28	5·18
All ages	1110	159·04	11·75	7·39	81·21	6·16	7·59	3022	721·80	23·88	46·81	10·01	21·38	73·83	6·70	9·07

S. D. = standard deviation. C. = coefficient of variation.

TABLE II.

Manchester Grammar School Data. Total Correlation Coefficients.

Variables	11 years (29)	12 years (112)	13 years (173)	14 years (255)	15 years (263)	16 years (173)	17 years (80)	18 years (25)	12—17 years Mean value	All ages (1110)
Weight and standing height ...	·789 ± ·047	·748 ± ·028	·788 ± ·019	·774 ± ·017	·823 ± ·013	·812 ± ·017	·704 ± ·038	·784 ± ·052	·787	·897 ± ·004
Weight and stem length ...	·967 ± ·008	·709 ± ·032	·754 ± ·022	·819 ± ·014	·830 ± ·013	·749 ± ·023	·760 ± ·032	·718 ± ·065	·783	·901 ± ·004
Weight and vital capacity ...	·395 ± ·106	·635 ± ·038	·760 ± ·022	·693 ± ·022	·785 ± ·016	·736 ± ·024	·590 ± ·049	·778 ± ·053	·720	·851 ± ·006
Standing height and vital capacity	·457 ± ·099	·578 ± ·042	·732 ± ·024	·733 ± ·020	·718 ± ·020	·682 ± ·027	·577 ± ·050	·715 ± ·066	·692	·835 ± ·006
Stem length and vital capacity...	·234 ± ·118	·551 ± ·044	·788 ± ·019	·733 ± ·020	·683 ± ·022	·674 ± ·028	·566 ± ·051	·783 ± ·052	·688	·836 ± ·006
Standing height and stem length	·490 ± ·095	·853 ± ·017	·878 ± ·012	·886 ± ·009	·825 ± ·013	·851 ± ·014	·808 ± ·026	·822 ± ·044	·854	—
Standing height and chest circumference ...	·225 ± ·119	·652 ± ·037	·670 ± ·028	·651 ± ·024	·688 ± ·022	·651 ± ·030	·507 ± ·056	·373 ± ·116	·653	·836 ± ·006
Stem length and chest circumference ...	·421 ± ·103	·716 ± ·031	·646 ± ·030	·710 ± ·021	·718 ± ·020	·669 ± ·028	·629 ± ·045	·487 ± ·103	·689	·835 ± ·006
Vital capacity and chest circumference ...	·419 ± ·103	·550 ± ·044	·698 ± ·026	·699 ± ·025	·742 ± ·019	·694 ± ·027	·688 ± ·040	·552 ± ·094	·692	·831 ± ·006
Weight of body and chest circumference ...	·701 ± ·064	·881 ± ·014	·855 ± ·013	·877 ± ·010	·891 ± ·009	·868 ± ·013	·840 ± ·022	·780 ± ·053	·873	·936 ± ·002

positively correlated with increase of age, as considerable growth in all dimensions takes place in this age period and this correlation is not allowed for in calculating the total coefficients for all ages together. The correlation coefficients between the several dimensions and increasing age by half years were determined later to show how each increases with advancing age. They are shown in the first row of Table III. Knowing these values it was possible to calculate the partial correlations that existed between the several pairs of dimensions under consideration when age was assumed to remain constant (Table III). This procedure results in coefficients which are appreciably less in value than those determined for the corresponding pairs of dimensions irrespective of age, thus confirming the suggestion, made above, that varying age exerts an appreciable influence in determining the magnitude of the latter coefficients.

In reviewing the values of the coefficients of correlation, as they are presented in Table II, several points of interest become evident. The coefficients between any two variables seem to show definite, if not significant, differences at the several ages. This is marked even when the coefficients for ages 11 and 18 are excluded owing to the paucity of observations on which they are based. While there is no very definite evidence that they vary at the different ages in any special or characteristic manner it seems more than a coincidence that, with one or two exceptions, the highest values are attained at ages 13 to 15*. This relationship seems to merit further investigation. The highest correlation found is that between weight of body and chest circumference which just exceeds in value that between standing height and stem or trunk length. One notable feature is the relative degree of association shown between the standing height and stem length respectively and the other variables. The correlation coefficients between standing height and weight and stem length and weight are practically identical, and the same holds for standing height and vital capacity and stem length and vital capacity and also for standing height and chest circumference and stem length and chest circumference. The full stature measurement has recently fallen into disrepute and there has been a tendency, for various theoretical reasons, to discard it practically as being a measurement of no great significance in relation to other body measurements. It has been suggested that, as compared with the stem length, full stature is an "impure measure" owing to the varying angle of the neck of the femur, varying spinal curvature, varying compression of the intervertebral discs, varying thickness and turgescence of the soft tissues in the scalp and soles, deformities or defective growth in the leg bones and other causes, while it has been asserted confidently by recent workers on the subject that no simple relationship can be found to exist between standing height and various other body measurements, including vital capacity, while such a relationship can be demonstrated

* The mean coefficients for all characters run

Age	12	13	14	15	16	17
Mean coefficient	·6873	·7569	·7575	·7703	·7386	·6669

TABLE III.

Partial Correlation Coefficients. Boys of all Ages.

	Vital capacity and standing height	Vital capacity and stem length	Vital capacity and chest girth	Vital capacity and weight	Vital capacity and age	Standing height and chest girth	Stem length and chest girth
Total Coefficients ...	·835 ± ·006	·836 ± ·006	·831 ± ·006	·851 ± ·006	·662 ± ·011	·836 ± ·006	·835 ± ·006
Partial Coefficients							
(*First Order*)							
Standing height constant	—	—	·441 ± ·016	·419 ± ·017	·171 ± ·020	—	—
Stem length constant ...	—	—	·440 ± ·016	·411 ± ·017	·148 ± ·020	—	—
Chest constant	·459 ± ·016	·464 ± ·016	—	·374 ± ·017	·188 ± ·020	—	—
Weight constant	·310 ± ·018	·304 ± ·018	·184 ± ·020	—	·175 ± ·020	−·026 ± ·020	−·054 ± ·020
Vital capacity constant...	—	—	—	—	—	·464 ± ·016	·460 ± ·016
Age constant	·690 ± ·011	·690 ± ·011	·684 ± ·011	·724 ± ·009	—	·669 ± ·011	·661 ± ·011
(*Second Order*)							
Standing height and chest constant	—	—	—	·140 ± ·020	·051 ± ·020	—	—
Stem length and chest constant	—	—	—	·126 ± ·020	·034 ± ·020	—	—
Standing height and weight constant ...	—	—	·206 ± ·019	—	·099 ± ·020	—	—
Stem length and weight constant	—	—	·213 ± ·019	—	091 ± ·020	—	—
Standing height and age constant	—	—	·416 ± ·017	·399 ± ·017	—	—	—
Stem length and age constant	—	—	·420 ± ·017	·397 ± ·017	—	—	—
Chest and weight constant	·318 ± ·018	·320 ± ·018	—	—	·182 ± ·020	—	—
Chest and age constant...	·429 ± ·017	·433 ± ·016	—	·355 ± ·018	—	—	—
Weight and age constant	·271 ± ·019	·267 ± ·019	·155 ± ·020	—	—	−·087 ± ·020	−·125 ± ·020
Standing height and vital capacity constant	—	—	—	—	—	—	—
Stem length and vital capacity constant	—	—	—	—	—	—	—
Chest and vital capacity constant	—	—	—	—	—	—	—
Weight and vital capacity constant	—	—	—	—	—	−·086 ± ·020	−·119 ± ·020
Age and vital capacity constant	—	—	—	—	—	·371 ± ·018	·358 ± ·018
(*Third Order*)							
Standing height, chest and weight constant	—	—	—	—	·058 ± ·020	—	—
Stem length, chest and weight constant	—	—	—	—	·043 ± ·020	—	—
Standing height, chest and age constant	—	—	—	·143 ± ·020	—	—	—
Stem length, chest and age constant	—	—	—	·129 ± ·020	—	—	—
Standing height, weight and age constant	—	—	·190 ± ·020	—	—	—	—
Stem length, weight and age constant	—	—	·198 ± ·020	—	—	—	—
Standing height, age and vital capacity constant	—	—	—	—	—	—	—
Stem length, age and vital capacity constant	—	—	—	—	—	—	—
Chest, weight and age constant	·293 ± ·019	·292 ± ·019	—	—	—	—	—
Weight, vital capacity and age constant	—	—	—	—	—	−·133 ± ·020	−·174 ± ·020
Chest, vital capacity and age constant	—	—	—	—	—	—	—

TABLE III (*continued*).

	Standing height and weight	Stem length and weight	Weight and chest girth	Standing height and age	Stem length and age	Chest girth and age	Weight and age
Total Coefficients ...	·897 ± ·004	·901 ± ·004	·936 ± ·003	·714 ± ·010	·725 ± ·010	·708 ± ·010	·701 ± ·010
Partial Coefficients (First Order)							
Standing height constant	—	—	·767 ± ·008	—	—	—	—
Stem length constant ...	—	—	·770 ± ·008	—	—	—	—
Chest constant	·595 ± ·013	·617 ± ·013	—	—	—	—	—
Weight constant... ...	—	—	—	—	—	—	—
Vital capacity constant...	·644 ± ·012	·658 ± ·012	·783 ± ·008	—	—	—	—
Age constant	·794 ± ·008	·800 ± ·007	·873 ± ·005	—	—	—	—
(*Second Order*)							
Standing height and chest constant	—	—	—	—	—	—	—
Stem length and chest constant	—	—	—	—	—	—	—
Standing height and weight constant ...	—	—	—	—	—	—	—
Stem length and weight constant	—	—	—	—	—	—	—
Standing height and age constant	—	—	·757 ± ·009	—	—	—	—
Stem length and age constant	—	—	·764 ± ·008	—	—	—	—
Chest and weight constant	—	—	—	—	—	—	—
Chest and age constant...	·583 ± ·013	·607 ± ·013	—	—	—	—	—
Weight and age constant	—	—	—	—	—	—	—
Standing height and vital capacity constant	—	—	·716 ± ·010	—	—	—	—
Stem length and vital capacity constant	—	—	·719 ± ·010	—	—	—	—
Chest and vital capacity constant	·513 ± ·015	·540 ± ·014	—	—	—	—	—
Weight and vital capacity constant	—	—	—	—	—	—	—
Age and vital capacity constant	·588 ± ·013	·601 ± ·013	·752 ± ·009	—	—	—	—
(*Third Order*)							
Standing height, chest and weight constant	—	—	—	—	—	—	—
Stem length, chest and weight constant	—	—	—	—	—	—	—
Standing height, chest and age constant	—	—	—	—	—	—	—
Stem length, chest and age constant	—	—	—	—	—	—	—
Standing height, weight and age constant	—	—	—	—	—	—	—
Stem length, weight and age constant	—	—	—	—	—	—	—
Standing height, age and vital capacity constant	—	—	·711 ± ·010	—	—	—	—
Stem length, age and vital capacity constant	—	—	·717 ± ·010	—	—	—	—
Chest, weight and age constant	—	—	—	—	—	—	—
Weight, vital capacity and age constant	—	—	—	—	—	—	—
Chest, vital capacity and age constant	·505 ± ·015	·537 ± ·014	—	—	—	—	—

between these measurements and stem length. The values in Table II show, however, that, so far as a conclusion can be drawn for the period of life covered by the age of these boys, the standing height is in all respects as closely related to weight, to chest girth and to vital capacity as the stem length. The very close similarity in the relationships of standing height and stem length respectively to vital capacity is shown more precisely by the values of the partial correlation coefficients of the different orders between the variables when the age and other measurements are assumed to remain constant. These are shown in the first two columns of Table III. It is evident that the corresponding values of the coefficients in the two cases are practically identical. Further proof of this similarity in relationship will be furnished later. Table II shows also the degree of correlation existing between body weight and chest girth respectively and vital capacity. It is seen that, of the measurements that have been taken, body weight is most highly correlated with the vital capacity, the magnitude of this correlation even exceeding slightly, though perhaps not to a significant degree, the value shown for that between chest girth and vital capacity. While the last two dimensions show a well marked correlation, increase of chest circumference being closely related on the average to increased vital capacity, yet the higher value shown in the correlation coefficient between weight and vital capacity would appear to indicate to what an extent in the growing boy the magnitude of the record of the spirometer is dependent on the increase of body weight. Increased weight implies vigour and power to expel the air from the chest, and this appears to be as important in determining a better vital capacity as variation in the actual size of the chest. This view seems to be supported by the circumstance that the partial correlation between weight and vital capacity, when the chest girth is assumed to remain constant, is distinctly higher than that between the chest circumference and vital capacity with constant weight, the values being 0·374 and 0·184 respectively (Table III). In the partial correlations of the highest order, however, the position is reversed. Chest girth appears to be more highly correlated with vital capacity than is weight when all the other variables are assumed to remain constant. The difference is slight, and the balance in favour of chest girth cannot be regarded as of practical significance. The correlation that exists between the weight-height ratio and increasing age in half years has also been calculated. The value of the coefficient is 0·630, which is certainly significant and indicates that, with advancing years during the period of adolescence, weight definitely increases relatively to height.

Regression Equations. The correlation ratios between weight and stem length, weight and full stature, weight and vital capacity, full stature and vital capacity, stem length and vital capacity and between these several measurements and age were calculated and are shown in Table IV. The values of $\frac{\sqrt{N}}{\cdot 67449}\,\frac{1}{2}\sqrt{\zeta}$, the approximate test used, seemed to indicate that, within the range of observation, there was not in the case of any of the variates, with the exception of full stature, any very marked departure from linearity of regression on age. The regression of full stature on age is known to be non-linear. In regard to the regression on the other

measurements full stature also shows the greatest departure from linearity. Graphs were drawn (Diagrams 1—5) showing the regression straight line in relation to

TABLE IV.

Manchester Grammar School Data. Correlation Ratios.

Showing Deviation from Linearity in Regression Lines.

Variables	Correlation ratio η	Correlation coefficient r	$\eta^2 - r^2$ (z)	$\frac{\sqrt{N}}{\cdot 67449} \frac{1}{2}\sqrt{z}$
Weight on age	·709	·702	·011280	2·63
Vital capacity on age ...	·669	·662	·009317	2·37
Full stature on age	·726	·714	·017280	3·25
Stem length on age	·733	·725	·011664	2·67
Chest girth on age	·715	·708	·009961	2·47
Weight on full stature ...	·902	·897	·008995	2·35
Full stature on weight ...	·908		·019855	3·50
Weight on stem length ...	·904	·901	·005415	1·82
Stem length on weight ...	·907		·010848	2·58
Vital capacity on weight ...	·855	·851	·006824	2·03
Weight on vital capacity ...	·863		·020568	3·53
Vital capacity on full stature	·841	·835	·010056	2·47
Full stature on vital capacity	·855		·033800	4·53
Vital capacity on stem length	·839	·836	·005025	1·75
Stem length on vital capacity	·851		·025305	3·92

the means of the arrays for each of the five measurements on age. The straight lines seem to give a reasonably good fit except at the extremities of the range where the mean values of the arrays are based on very few observations. The "goodness of fit" test gives the following values:

	χ^2	P
Weight on age	25·929	·102
Vital capacity on age ...	24·647	·136
Chest on age	21·920	·236
Stem length on age ...	31·420	·036
Full stature on age ...	39·721	·002

indicating clearly that the fits obtained for stem length and full stature are not good when all the observations are included. As the appearance of the graph for full stature on age suggested that the deviations were not random and that a better fit would be obtained by fitting a cubic parabola to the data, this was done for each of the measurements. In each case the value at age 10·25 was excluded to obtain an uneven number of observations. The cubic equations calculated are shown in the Diagrams 1—5 and the fits obtained are indicated by the interrupted lines. The accuracy of the fit obtained by the cubic equation may be compared

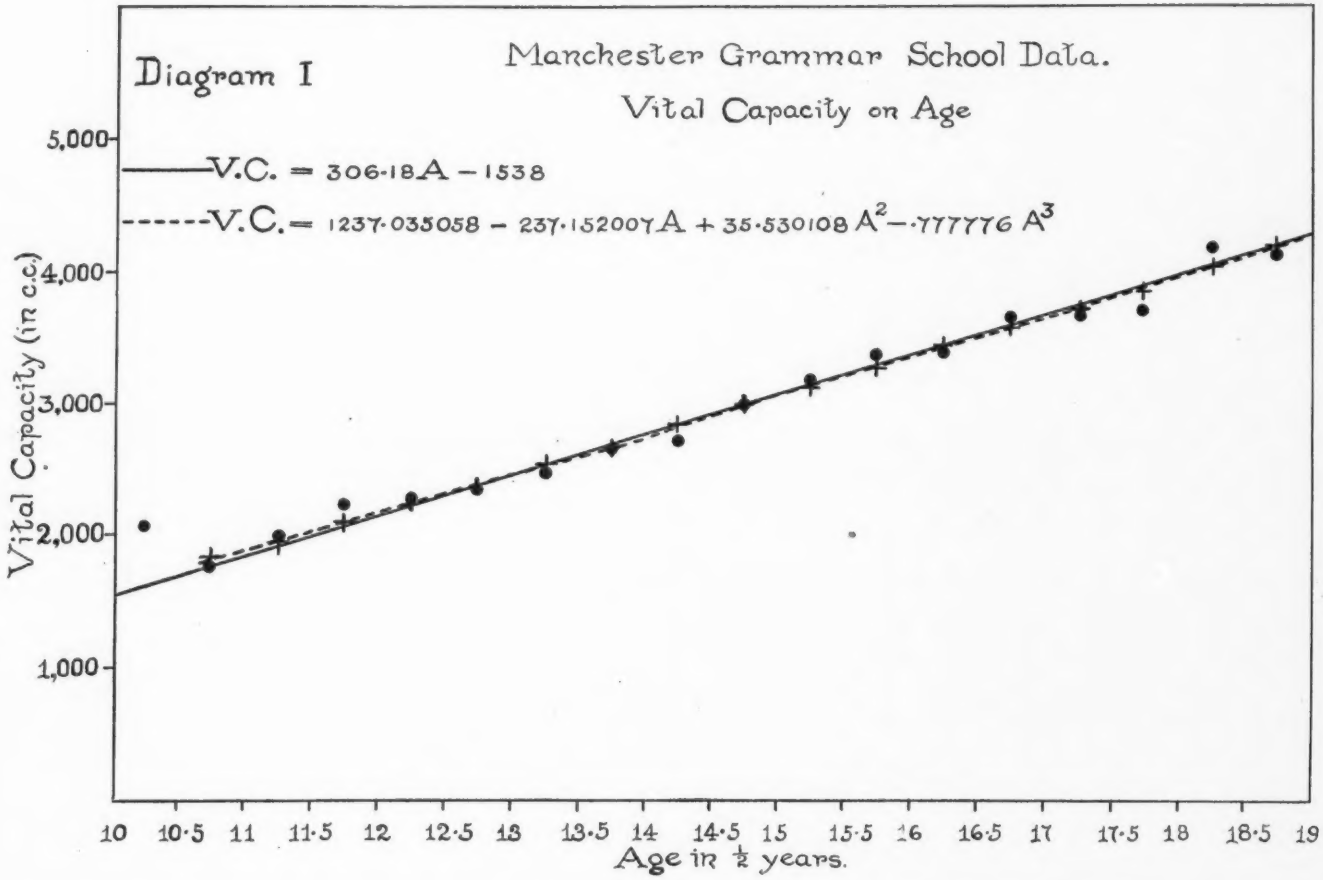
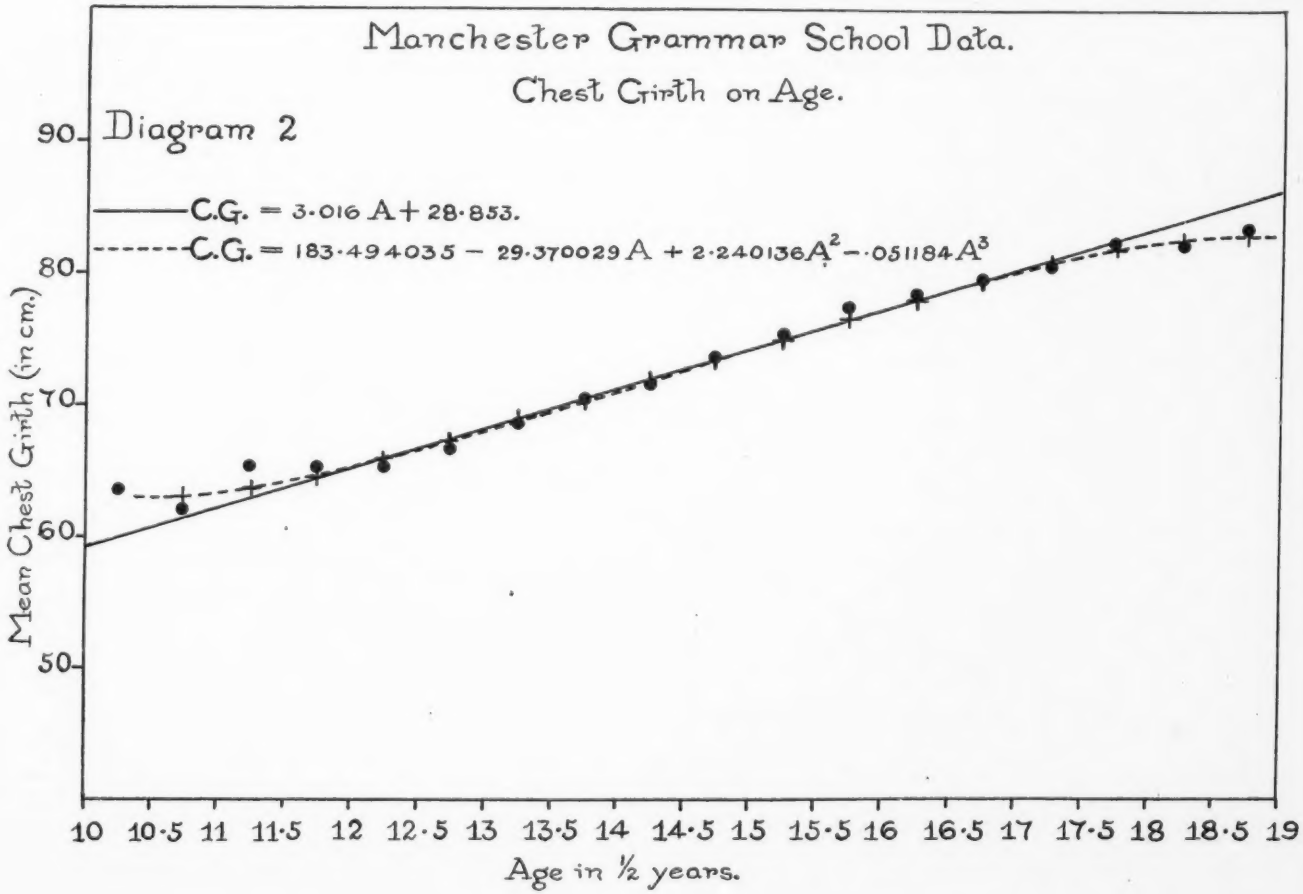


Diagram I
Manchester Grammar School Data.
Vital Capacity on Age
V.C. = 306·18A − 1538
V.C. = 1237·035058 − 237·152007A + 35·530108 A² − ·777776 A³
Vital Capacity (in c.c.)
5,000
4,000
3,000
2,000
1,000
10 10·5 11 11·5 12 12·5 13 13·5 14 14·5 15 15·5 16 16·5 17 17·5 18 18·5 19
Age in ½ years.

Age in ½ years.



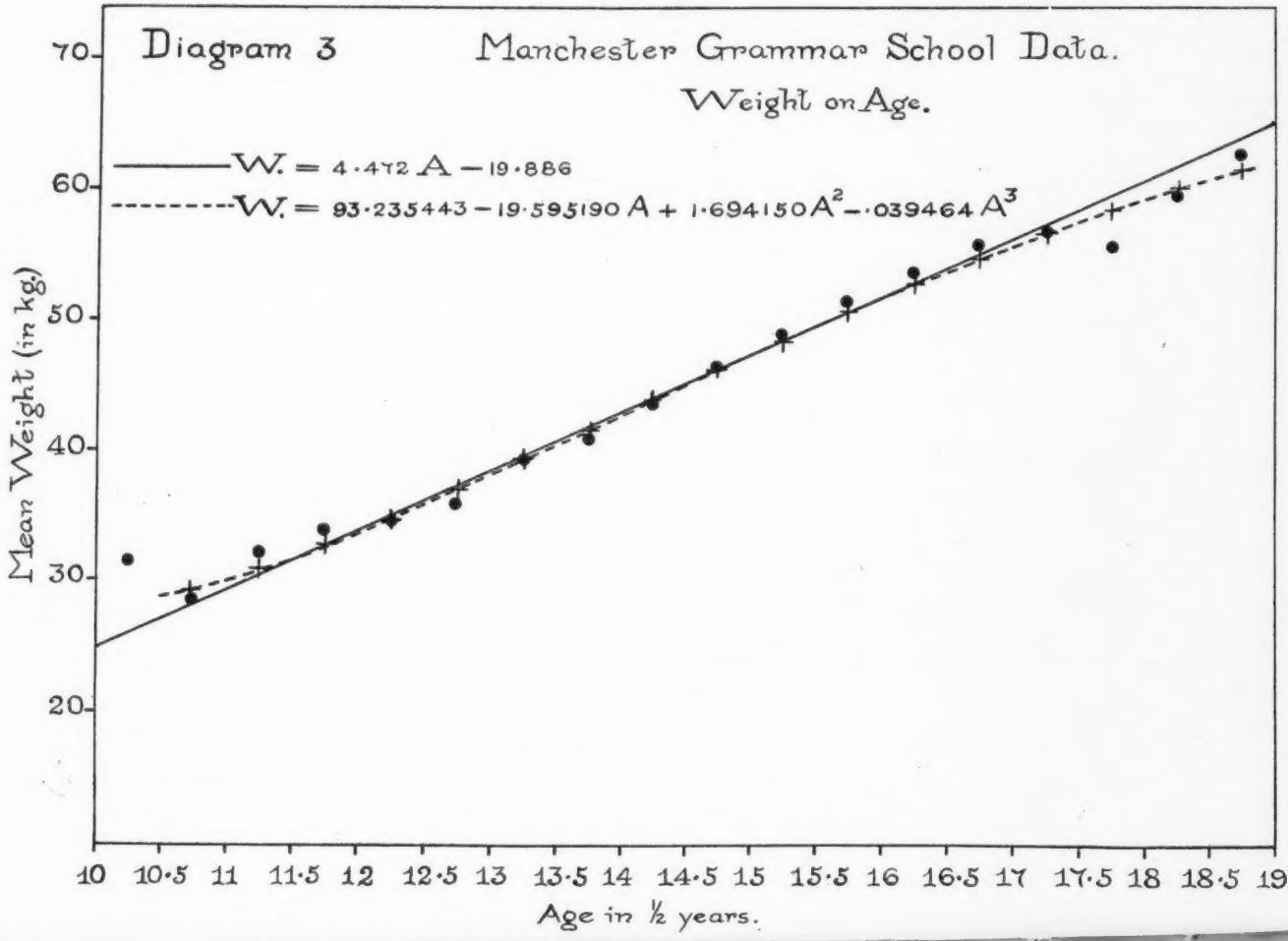
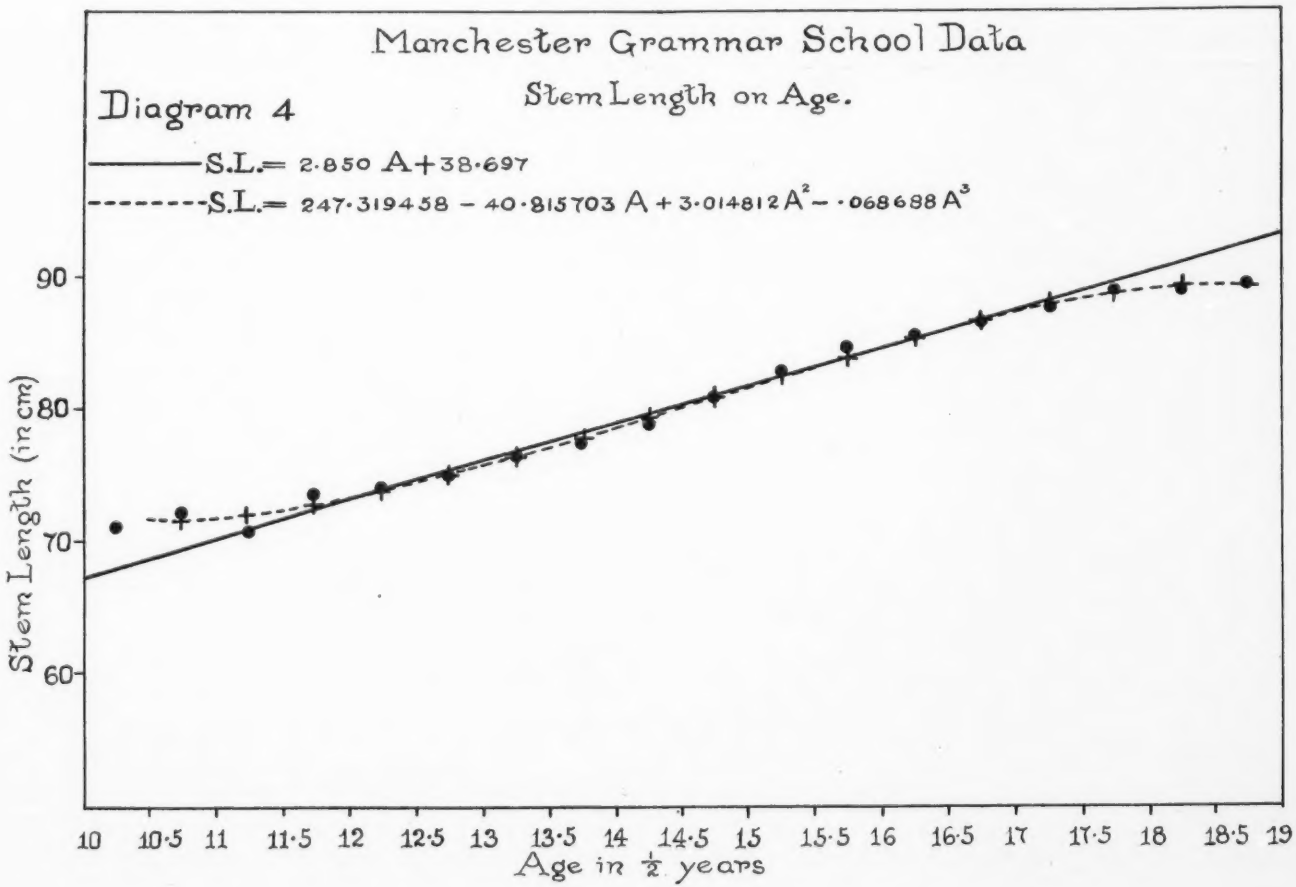


Diagram 3
Manchester Grammar School Data.
Weight on Age.
W. = 4·472 A − 19·886
W. = 93·235443 − 19·595190 A + 1·694150 A² − ·039464 A³
Mean Weight (in kg)
70
60
50
40
30
20
10 10·5 11 11·5 12 12·5 13 13·5 14 14·5 15 15·5 16 16·5 17 17·5 18 18·5 19
Age in ½ years.

Age in ½ years.



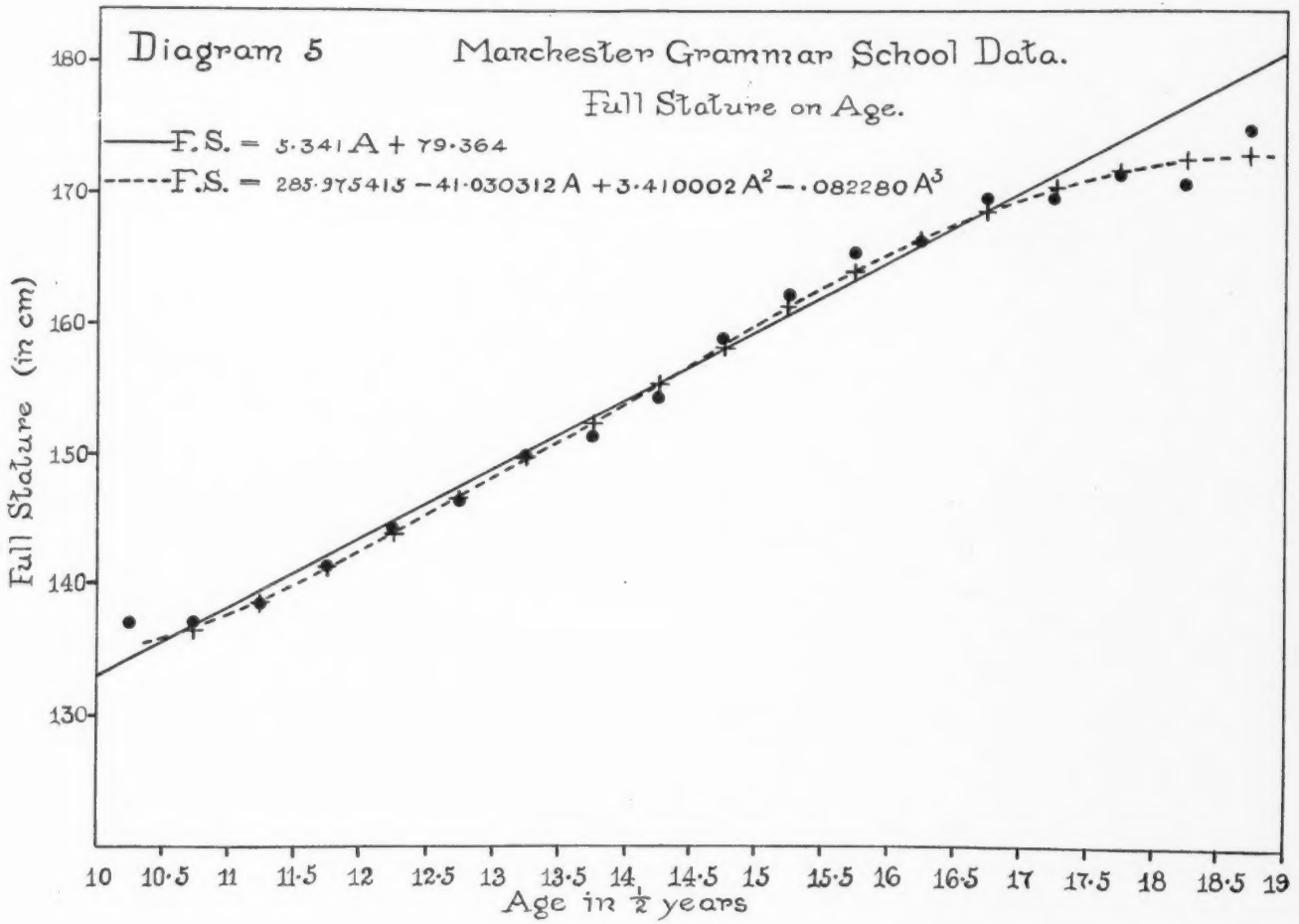


Diagram 5
Manchester Grammar School Data.
Full Stature on Age.
F.S. = 5·341 A + 79·364
F.S. = 285·975415 − 41·030312 A + 3·410002 A² − ·082280 A³
Full Stature (in cm)
180
170
160
150
140
130
10 10·5 11 11·5 12 12·5 13 13·5 14 14·5 15 15·5 16 16·5 17 17·5 18 18·5 19
Age in ½ years

with that obtained by linear regression in the several cases by tabulating the root-mean-square deviations obtained from the corresponding 17 observations.

	Cubic parabola	Regression straight line (omitting first observation)
Vital capacity on age ...	85·55 c.c.	89·20 c.c.
Weight on age	1·05 kilog.	1·38 kilog.
Stem length on age ...	·44 cm.	1·17 cm.
Chest girth on age ...	·58 cm.	1·05 cm.
Full stature on age ...	·95 cm.	2·18 cm.

The cubic equation thus apparently gives a decidedly better fit than the regression line and the improvement is greatest in the two measurements of length. As the regression lines seemed to give moderately good fits except at the extreme ages the coefficients of regression and regression equations were determined for the several pairs of variables. These equations are shown in Table V, with the standard

TABLE V. *Manchester Grammar School Data. Regression Formulae.*

			Standard error of regression equation	Percentage error
1	All ages	$X_1 = 97{\cdot}8636X_2 - 4922{\cdot}8322$	396·9 c.c.	13·13
2	„	$X_1 = 51{\cdot}3839X_0 - 5148{\cdot}4497$	397·1 c.c.	13·14
3	„	$X_1 = 61{\cdot}4204X_3 + 148{\cdot}1111$	379·4 c.c.	12·55
4	„	$X_1 = 89{\cdot}6538X_4 - 3590{\cdot}0781$	401·3 c.c.	13·28
5	„	$X_1 = 21{\cdot}65X_0 + 18{\cdot}00X_3 + 29{\cdot}63X_4 + 19{\cdot}47X_5 - 3740{\cdot}67$	352·5 c.c.	11·66
6	„	$X_1 = 42{\cdot}71X_2 + 16{\cdot}60X_3 + 31{\cdot}16X_4 + 14{\cdot}57X_5 - 3741{\cdot}03$	352·5 c.c.	11·66
7	Age 12	$X_1 = 23{\cdot}58X_2 + 45{\cdot}09X_3 - 14{\cdot}16X_4 + {\cdot}48$	301·0 c.c.	12·44
8	„ 13	$X_1 = 63{\cdot}75X_2 + 17{\cdot}48X_3 + 20{\cdot}00X_4 - 4394{\cdot}73$	258·0 c.c.	10·01
9	„ 14	$X_1 = 58{\cdot}95X_2 - 6{\cdot}24X_3 + 43{\cdot}98X_4 - 4755{\cdot}29$	323·0 c.c.	11·31
10	„ 15	$X_1 = 15{\cdot}22X_2 + 37{\cdot}86X_3 + 24{\cdot}26X_4 - 1757{\cdot}69$	349·0 c.c.	10·68
11	„ 16	$X_1 = 37{\cdot}43X_2 + 27{\cdot}81X_3 + 24{\cdot}96X_4 - 3193{\cdot}03$	404·0 c.c.	11·52
12	„ 17	$X_1 = 56{\cdot}00X_2 - 17{\cdot}68X_3 + 97{\cdot}09X_4 - 8066{\cdot}99$	475·0 c.c.	12·79
13	All ages	$X_3 = 1{\cdot}4643X_2 - 72{\cdot}0369$	4·36 klg.	9·31
14	„	$X_3 = {\cdot}7658X_0 - 74{\cdot}9083$	4·43 klg.	9·46
15	„	$X_3 = 1{\cdot}3974X_4 - 56{\cdot}3204$	3·53 klg.	7·54

where X_1 = Vital capacity in cubic centimetres.
X_0 = Standing height in centimetres.
X_2 = Stem length in centimetres.
X_3 = Weight in kilogrammes.
X_4 = Chest girth in centimetres.
X_5 = Age in years.

errors (or root-mean-square errors) made in using them. The coefficients of regression show the change in one variable that is associated on the average with unit change in the other and from the equations the average values of one variable can be predicted from a knowledge of the values of the other. It is interesting to note that the regression coefficient between standing height and vital capacity (changed to inches and cubic inches), calculated from the 1100 observations, indi-

cates that for these boys, ranging in age from 11 to 18 years, an increase of one inch in height is associated on the average with an increase of 7·97 cubic inches in vital capacity, an unexpected confirmation of the conclusion arrived at by Hutchinson* so many years ago, but recently discredited, namely, that "for every inch of height (from 5–6 ft.) eight additional cubic inches of air at 60° are given out by a forced expiration." Hutchinson's dictum as to the occurrence of an increase in vital capacity of eight cubic inches with increase in height of one inch cannot be expected to hold from case to case with unfailing accuracy; it was only intended to be applicable to typical cases or to cases on the average. A similar relationship seems to obtain for adults; at least, it is shown in Arnold's data, to which further reference will be made later. In these the coefficient of regression of vital capacity on height is 51·58 c.c. for each cm. in height, which, converted into cubic inches and inches, indicates that for an increase of one inch in height there is, on the average, an increase of 7·99 cubic inches of vital capacity.

It is important to note the magnitude of the standard errors of the primary regression equations or standard deviations of the arrays shown in Table V. These are for boys of all ages and are seen to exceed 9 per cent., in estimating weight from stem length or full length, and 13 per cent., in estimating vital capacity from either stem length, full length or chest girth. For the groups of boys at each age from 12–17, not shown in the table, the standard deviations of weight arrays on stem length vary from 8 to 10·5 per cent., and of vital capacity arrays on weight from 11 to 14·5 per cent. In addition to these primary regression formulae, multiple regression formulae suitable for the prediction of vital capacity from the other measurements in the Grammar School boys were subsequently calculated. These give the vital capacity,

1. in terms of the stem length, chest girth, weight and age for boys at all ages;
2. in terms of the stem length, chest girth and weight for boys at each age from 12–17 years inclusive;
3. in terms of full stature, chest girth, weight and age for boys at all ages.

These formulae with the root-mean-square errors made in using them are also shown in Table V. It is evident that in thus predicting vital capacity from the partial regression equations including all the variables the average percentage errors remain still in excess of 10 per cent.

Correlation shown in other Data. It seemed desirable at this stage for the purposes of comparison and confirmation to inquire into the relationships that obtain between the corresponding measurements in data for the same and other periods of life. For that purpose, use was made of certain data which were kindly placed at our disposal by Dr Hobson. These included the measurements of:

(*a*) 100 adults from the police force.

(*b*) 279 boys of ages 12–18 years from Westminster School.

(*c*) 215 boys of ages 5–13 years from the L.C.C. School, Ellerslie Road.

* Hutchinson, John (1846), "On the Capacity of the Lungs and on the Respiratory Functions, etc." *Medico-Chirug. Trans.* XXIX. London. Ibid. (1846), *The Lancet*, Vol. I.

TABLE VI.

Dr Hobson's Data. Mean Values.

Data	Standing Height			Stem Length			Weight			Vital Capacity			Chest Girth			Age		
	Mean cm.	S. D. cm.	C.	Mean cm.	S. D. cm.	C.	Mean kilog.	S. D. kilog.	C.	Mean c.c.	S. D. c.c.	C.	Mean cm.	S. D. cm.	C.	Mean yrs	S. D. yrs	C.
Boys of L.C.C. School 5–13 years (215)	130·27	14·00	10·75	69·06	5·42	7·85	26·94	7·06	26·21	2000	486·20	24·31	61·99	5·20	8·39	10·19	2·64	25·91
Boys of Westminster School, 12–13 years (279)	168·48	10·18	6·04	85·16	5·78	6·79	52·90	9·80	18·53	3739	787·50	21·06	79·84	6·31	7·90	15·87	1·40	8·82
Selected adults from police force (100)	179·10	4·39	2·45	92·59	2·68	2·89	70·76	6·03	8·52	4823	492·00	10·20	88·92	4·20	4·72	—	—	—
Boys of Manchester Grammar School, 11–18 years (1100). For comparison with Westminster boys	159·04	11·75	7·39	81·21	6·16	7·59	46·81	10·01	21·38	3022	721·80	23·88	73·83	6·70	9·07	14·91	1·57	10·53

S. D. = standard deviation. C. = coefficient of variation.

The mean values of the measurements in the respective groups with their standard deviations and coefficients of variation are shown in Table VI. The total and partial correlation coefficients that have been calculated for the data are also shown in Table VII. The correlations obtained between full length and stem

TABLE VII.

Dr Hobson's Data and Arnold's Data.

Total and Partial Correlation Coefficients.

Variables	100 selected adults from police force	279 boys of Westminster School. Age 12–18 years	215 boys of L.C.C. School. Age 5–13 years	Arnold's data, 116 males. Age 17–30 years
	r	r	r	r
Standing height and weight ...	+0·428 ± ·055	+0·842 ± ·012	+0·944 ± ·005	·615 ± ·040
Stem length and weight	+0·341 ± ·060	+0·863 ± ·010	+0·908 ± ·008	—
Standing height and vital capacity	+0·292 ± ·062	+0·807 ± ·014	+0·878 ± ·011	·705 ± ·032
Stem length and vital capacity ...	+0·321 ± ·061	+0·827 ± ·013	+0·817 ± ·015	·593 ± ·041
Weight and vital capacity	+0·190 ± ·065	+0·843 ± ·012	+0·874 ± ·011	·524 ± ·046
Chest girth and vital capacity ...	+0·150 ± ·066	+0·849 ± ·011	+0·784 ± ·018	—
Chest girth and weight	+0·619 ± ·042	+0·917 ± ·006	+0·927 ± ·006	—
Standing height and stem length ...	+0·611 ± ·042	+0·917 ± ·006	+0·942 ± ·005	·723 ± ·030
Standing height and chest girth ...	+0·038 ± ·067	+0·780 ± ·016	+0·888 ± ·010	—
Stem length and chest girth ...	+0·155 ± ·066	+0·816 ± ·013	+0·849 ± ·013	—
Standing height and age	—	+0·630 ± ·024	+0·901 ± ·008	—
Stem length and age	—	+0·678 ± ·022	+0·822 ± ·015	—
Weight and age	—	+0·658 ± ·023	+0·865 ± ·012	—
Vital capacity and age	—	+0·669 ± ·022	+0·804 ± ·016	—
Chest girth and age	—	+0·664 ± ·023	+0·816 ± ·015	—
Weight-stem length ratio and age ...	—	—	+0·850 ± ·012	—
Standing height and weight with age constant	—	+0·730 ± ·019	+0·760 ± ·019	—
Stem length and weight with age constant	—	+0·754 ± ·017	+0·689 ± ·026	—
Standing height and vital capacity with age constant	—	+0·669 ± ·022	+0·598 ± ·030	—
Stem length and vital capacity with age constant	—	+0·683 ± ·022	+0·461 ± ·036	—
Weight and vital capacity with age constant	—	+0·720 ± ·019	+0·601 ± ·029	—
Chest girth and vital capacity with age constant	—	+0·729 ± ·019	+0·373 ± ·039	—
Weight and chest girth with age constant	—	+0·853 ± ·011	+0·762 ± ·019	—
Stem length and chest girth with age constant	—	+0·666 ± ·022	+0·541 ± ·033	—
Standing height and vital capacity with weight constant	+0·238 ± ·064	—	—	·570 ± ·042
Weight and vital capacity with height constant	+0·075 ± ·067	—	—	·161 ± ·061

length respectively and weight and vital capacity seem, on the whole, to support the conclusions drawn from the data for the boys of the Grammar School. In the boys of Westminster School, the correlation coefficients between stem length and the other two measurements appear to be slightly in excess of those shown between these and full length, but the differences are practically insignificant.

The standard deviations of the vital capacity array for the type of weight array and of the weight array for the type of stem length array have been calculated and are shown in Table VIII. For the two groups of boys, Westminster and

TABLE VIII.

Dr Hobson's Data.

Data	Standard deviation of weight arrays for stem length array: Mean weight in kilos.	S. D. of weight array in kilos.	Percentage S. D.	Standard deviation of vital capacity arrays for weight array: Mean V. C. in c.c.	S. D. of V. C. array in c.c.	Percentage S. D.
215 boys of L.C.C. School. Age 5–13 years	26·94	2·95	10·9	2000	236·2	11·8
279 boys of Westminster School. Age 12–18 yrs	52·90	4·95	9·4	3739	423·6	11·3
100 adults selected from the police force	70·76	5·67	8·0	4823	483·0	10·0

Ellerslie Road, the standard deviation of the vital capacity array is again in excess of 11 per cent., and that of weight exceeds 9 per cent. In the data for the group of adults belonging to the police force, the standard deviation of the vital capacity array is still as high as 10 per cent., and that of the weight array as high as 8 per cent. This shows the extent of variation in these two measurements that may be found even in such a carefully selected group of individuals.

For the later period of life, other data regarding the weight, the full stature, the stem length and the vital capacity were obtained from a paper published in 1855 by Arnold who was familiar with Hutchinson's work on the respiratory functions and was apparently greatly influenced by it in the methods of pursuing his investigations. For a series of observations on 116 males ranging in age from 17 to 30 years it was found that the correlation between standing height and vital capacity was 0·705, that between stem length and vital capacity 0·593, while that between standing height and stem length was 0·723 (Table VII). In this group also there is, therefore, no evidence that stem length is more closely correlated with vital capacity than is full stature, thus confirming by a modern method of analysis Arnold's direct conclusion from the study of his observations that he could not agree that stem length was of more value than full stature in estimating vital capacity. The correlation coefficient between weight and vital capacity was 0·524, and that between weight and height 0·615. The partial correlation between height and vital capacity when weight was assumed to remain constant was 0·570, while that between weight and vital capacity when height was constant was only 0·161, from which it would appear that in adults vital capacity is in closer relationship to full stature than to weight. In the data for the 100 adults selected from the police force, the correlation between height and vital capacity is also definitely higher than that between weight and vital capacity. This difference is brought out more clearly by the partial correlation coefficients. It will be recalled that the

total correlations obtained for the Grammar School boys examined seemed to suggest that of the two measurements—weight and height—the former was at least as closely associated with vital capacity as the latter. This appeared to be confirmed for other boys at the same age period by the correlations found in the data for Westminster School (Table VII). The partial correlation coefficients of the highest order show, however, that in the boys, as in adults, when all the other measurements are constant, height is more closely correlated with vital capacity than is weight. In this connexion it is worthy of note that Arnold concluded from his observations on adults that weight was to be considered in relation to vital capacity only in so far as it was influenced by height.

Weight-length Relationship shown by Power Formulae. We now come to consider the relation of stem length and standing height to weight as shown by a formula of the type proposed by Dr Ainley Walker and Professor Dreyer.

It is of the form:

$$l = kw^n,$$

where l = length, stem or full, in centimetres: w = weight in kilogrammes: and k is a constant.

From the series of weights and heights, in the Grammar School data, the values of n and k were calculated by the method of least squares for the boys in each age group and for all ages taken together. The results are set out in Table IX.

TABLE IX.

Manchester Grammar School Data.

Formula: $l = kw^n$.

Age group	Standing height and weight		Stem length and weight	
	n_1	k_1	n_2	k_2
11 years	·279	53·120	·213	34·502
12 „	·278	54·059	·251	30·402
13 „	·269	55·942	·245	31·090
14 „	·280	54·251	·284	28·377
15 „	.284	54·140	·306	25·288
16 „	·281	54·543	·289	27·029
17 „	·226	68·349	·282	28·131
18 „	·291	52·099	·279	28·364
All ages (weighted observations)	·310	48·467	·315	24·261
All ages (weight in grammes)...	·310	5·6944	·315	2·7536
All ages (mean values) ...	·274	55·425	·271	28·721

l = length, stem or full, in centimetres. w = weight in kilogrammes. k = a constant.

It is evident that the series of values for standing height and weight at the several ages are quite as uniform and regular as, or even more uniform than, those for stem length and weight. The mean values for the series of exponents for standing height and for sitting height are practically identical, being 0·274 and 0·271 respectively, while the constant for standing height is very approximately

double that for sitting height. When the values of n and k are calculated for all the boys, weighting the observations but ignoring age, the values obtained are 0·310 and 0·315 for standing height and stem length respectively with constants 48·467 and 24·261, the latter almost exactly in the ratio of two to one. These constants are for weight in kilogrammes. For weight in grammes the corresponding constants are 5·6944 and 2·7536. The formula—$l = 2{\cdot}7536w^{\cdot315}$—thus obtained, connecting stem length and weight, is very similar to that calculated by Professor Dreyer as suitable for all ages, namely, $l = 2{\cdot}6298w^{\cdot319}$.

Vital capacity and height relationship shown by power formulae. Formulae of a similar type have been calculated from the data to show the relation of stem length and standing height respectively to vital capacity at each year of age and for the group of all ages above 11. The number at age 11 was small and the values obtained for the constants were somewhat different from those found for the later years, apparently indicating that the two measures of stature at this early age are not in the same or such close relationship to vital capacity as in the later years, although difficulty in manipulating the spirometer may be responsible in some degree for the discrepancy. The values are shown in comparison in Table X

TABLE X.

Manchester Grammar School Data.

Formula: V. C. $= kl^n$.

Age group	Vital capacity and standing height		Vital capacity and stem length	
	n_1	k_1	n_2	k_2
12 years	2·245	·031854	2·080	·29326
13 „	2·444	·012028	2·802	·013381
14 „	2·252	·03207	2·302	·11811
15 „	2·590	·005921	2·359	·093903
16 „	2·569	·006670	2·358	·095625
17 „	2·561	·007036	2·600	·032098
18 „	2·512	·010103	2·643	·0290900
All ages (weighted observations)	2·700	·003372	2·612	·030537
All ages (mean values)	2·431	·013453	2·417	·072853

V. C. in cubic centimetres. l in centimetres.

and appear to support the view that the standing height exhibits as constant and as regular a relationship to vital capacity at the different ages as the stem length and that there is no apparent advantage in using stem length in preference to full stature in the construction of formulae with the view of determining the normal vital capacity for a given height. The average values for the exponents in the formulae for boys at the several ages are 2·431 and 2·417, almost identical, while those for weighted observations, ignoring age, are 2·700 and 2·612 respectively.

Accuracy of prediction by regression and power formulae. It seemed desirable to test and compare the accuracy of the predictions of the weight and vital capacity

(respectively from the full length and stem length) deduced from the regression equations and from the power formulae; the latter are of the type suggested by Professor Dreyer as already described, but are calculated from the same data as the regression equations. For this purpose eight cards were taken at random from those of the groups of boys in each half year of age from 11·6 to 18·6, making in all 112, a fair sample. For this rough test it was necessary to use the data from which the constants were calculated as no other data covering the same age period were available at the time. In the first place theoretical weight values were obtained from the regression formula connecting stem length and weight, and secondly from the power formula, determined for boys of all ages irrespective of the number at each age. The root-mean-square deviations of the observed from the predicted values were determined and were found to be 4·33 cm. and 5·00 cm. respectively, suggesting that the regression formula gives, if anything, the better fit (Table XI).

TABLE XI.

Manchester Grammar School Data. Test of Accuracy of Prediction of Weight and Vital Capacity by Different Formulae.

	By regression equation 1	By power formula 2
Weight from sitting height (112 observations) ...	$\Delta = 4{\cdot}33$ cm.	$\Delta = 5{\cdot}00$ cm.
Weight from standing height (112 observations) ...	$\Delta = 4{\cdot}57$ cm.	$\Delta = 4{\cdot}94$ cm.
Vital capacity from sitting height (104 observations)...	$\Delta = 452$ c.c.	$\Delta = 450$ c.c.
Vital capacity from standing height (104 observations)	$\Delta = 455$ c.c.	$\Delta = 446$ c.c.

where Δ = root-mean-square deviation.

Estimating the weight from the full stature by two similar formulae the root-mean-square deviations were 4·57 cm. and 4·94 cm. respectively. Comparison of these with the previous figures indicates that the accuracy in prediction of weight in the group of cases comprising in all about one-tenth of the total number of boys by a power formula of the type suggested by Professor Dreyer, when stem length is used, is practically identical with that obtained by using standing height. The fit for the weight obtained by means of the regression equation for stem length is apparently slightly better than that obtained from the equation for full stature, but the difference is really insignificant for practical purposes.

Vital capacity was then predicted from the stem length in the same sample but excluding those under 12 years (for the reason already given) by the regression equation and by the corresponding power formula. The correlation coefficient on which the regression equation was based was recalculated from the total boys excluding those under 11 years so that the two formulae, being derived from the same data, might be more truly comparable. The root-mean-square deviations from the original observations obtained by prediction with the two formulae—regression and power—were 452 c.c. and 450 c.c. respectively. The root-mean-square deviations, when vital capacity was predicted by the corresponding formulae

for full stature, were 455 c.c. and 446 c.c. respectively. It would thus appear that in the data examined, the regression equation for stem length is not inferior to the corresponding power formula as an instrument of prediction. Although the full stature regression equation does not give quite so good a fit for vital capacity as that obtained by the corresponding power formula, the difference may be considered practically insignificant. It is evident, moreover, from Table XI that the accuracy of prediction of vital capacity, as of weight, obtained by the power formula and by the regression formula for full stature is at least equivalent to that obtained by the corresponding formulae for stem length.

From the data for the 100 adults of the police force, the 279 boys of Westminster School and the 215 boys of the L.C.C. School, power formulae of the type already described have also been calculated by the method of least squares. The formulae evaluated show for each individual group and also for all these groups associated together the relationship between (1) standing height and weight and stem length and weight, (2) standing height and vital capacity and stem length and vital capacity, and (3) weight and vital capacity (Table XII).

On comparing the formulae obtained for the Westminster boys with those for the Grammar School boys, it is found that the formulae for stem length and weight are in closer agreement than those for full length and weight, the exponent in the full length formula for the Westminster boys showing a slight decrease in value. In the combined group, however, it is found that the exponent in the stem length formula is as much in defect of that given in Professor Dreyer's formula as the exponent in the full stature formula is in excess of it.

On comparing the Westminster and Grammar School formulae for stem length and vital capacity and full stature and vital capacity it is found that the exponents in the former are practically identical with those for the Grammar School. While the exponent of the full stature formula for the grouped observations remains more or less constant, the exponent of the corresponding stem length formula seems to show some degree of divergence from that evaluated for the Westminster data. Taking everything into consideration it would appear that the formulae for stem length are not more constant than those for full stature and cannot be regarded as in any way superior to the latter as instruments of prediction. In the power formula connecting vital capacity and weight derived from the Westminster data, the exponent is ·97, while in that for the grouped observations it remains as high as ·92. As the corresponding exponent in the formula for the Grammar School data is practically unity there appears to be a significant divergence in all these data from what is given in Professor Dreyer's formula as the best value, namely ·72.

Discussion of Results and Conclusions.

It now remains to discuss briefly the results that have been obtained and their bearing on the conclusions arrived at by others.

It would appear that, despite the definite tendency shown in recent work on the interrelationships of the physical measurements, to replace the full length by

TABLE XII.

Showing Power Formulae Calculated for other Data.

	Standing height and weight $l_1 = kw^n$	Stem length and weight $l_2 = kw^n$	Standing height and vital capacity V. C. $= kl_1^n$	Stem length and vital capacity V. C. $= kl_2^n$	Weight and vital capacity V. C. $= kw^n$
100 adults	$l_1 = 105{\cdot}63w^{\cdot124}$	$l_2 = 56{\cdot}973w^{\cdot114}$	V. C. $= 9{\cdot}3618l^{1{\cdot}202}$	V. C. $= 27{\cdot}652l^{1{\cdot}138}$	V. C. $= 1803{\cdot}8w^{\cdot229}$
279 boys of Westminster School. (Age 12–18)	$l_1 = 56{\cdot}744w^{\cdot275}$	$l_2 = 24{\cdot}475w^{\cdot315}$	V. C. $= {\cdot}00278l^{2{\cdot}749}$	V. C. $= {\cdot}028212l^{2{\cdot}650}$	V. C. $= 80{\cdot}709w^{\cdot965}$
215 boys of L.C.C. School. (Age 5–13)	$l_1 = 35{\cdot}744w^{\cdot395}$	$l_2 = 28{\cdot}003w^{\cdot276}$	V. C. $= {\cdot}028443l^{2{\cdot}279}$	V. C. $= {\cdot}00473l^{3{\cdot}043}$	V. C. $= 261{\cdot}37w^{\cdot606}$
Three groups taken together. (594 individuals)	$l_1 = 42{\cdot}219w^{\cdot346}$	$l_2 = 26{\cdot}211w^{\cdot297}$	V. C. $= {\cdot}003637l^{2{\cdot}700}$	V. C. $= {\cdot}0038061l^{3{\cdot}099}$	V. C. $= 95{\cdot}982w^{\cdot917}$
Manchester Grammar School boys	$l_1 = 48{\cdot}467w^{\cdot310}$	$l_2 = 24{\cdot}261w^{\cdot315}$	V. C. $= {\cdot}003372l^{2{\cdot}700}$	V. C. $= {\cdot}030537l^{2{\cdot}612}$	V. C. $= 47{\cdot}481w^{1{\cdot}079}$

where l_1 = Full length in centimetres.
l_2 = Stem length in centimetres.
w = Weight in kilogrammes.
V. C. = Vital capacity in cubic centimetres.
k = a constant.

the stem length, the latter does not possess the many advantages over the former that have been frequently claimed or asserted. It would almost appear to be a matter of indifference as to which measurement of length is used.

In the data examined there is no evidence that the accuracy of prediction of vital capacity obtainable by a power formula of the type suggested is significantly greater than that obtained by the ordinary regression formula.

Professor Dreyer's formulae were originally deduced from observations on 16 individuals, men and boys ranging in age from 13 to 52 years, "who were carefully selected on account of their physical fitness and covered as widely different a range in weight, height, etc., as possible." Apart from the number of observations being so relatively small, that it is totally inadequate to form the basis of any general conclusion or to provide data for calculating a formula for general application, the process of selection adopted gives the formula so devised a wholly fictitious degree of accuracy as expressed in the tables by percentage error between calculated and observed values. The individuals chosen are more or less mean types lying in fairly close relationship to the best line drawn through the observations, and insufficient account is taken of the wide range of weights or of vital capacities found in individuals of, say, a similar height who are nevertheless normal and healthy. How great this variation may be, even in individuals at the same age and more especially for all ages taken together, has been shown by the magnitude of the standard deviation of the arrays of weights or vital capacities for height of a certain type found in the data for the boys examined. In these boys standard deviations of over 13 per cent. for the vital capacity array and over 9 per cent. for the weight array have been found, and yet apparently there is no reason to describe them as abnormal. With a range of variation in the measurements so extensive as that described, it is difficult to fix the percentage deviations from the standards,—or values found by the formulae from the other measurements,—that may reasonably be considered to differentiate the normal from the abnormal with sufficient precision or within sufficiently narrow limits to be of practical use, for example, in the early diagnosis of disease. It seems very doubtful, indeed, if some of the percentage deviations that have been given for certain formulae are adequate, and further investigation would appear to be necessary before they can be accepted without considerable reservation.

As the observations for vital capacity can be fitted with fair approximation to the Gaussian curve of errors it may serve to emphasise the wide range of variation that exists in normal data like those examined if it is stated that roughly 30 per cent. of the observations lie outside the standard deviation. This can easily be ascertained by reference to the table of the probability integral.

ÜBER EIN NEUES VERFAHREN BEI ANWENDUNG DER "VARIATE-DIFFERENCE"-METHODE*.

VON OSKAR ANDERSON.

EINE evolutorische oder säkulare Reihe, wie sie in der statistichen Praxis vorkommt, kann gewöhnlich als die Summe einer "glatten" Grundreihe und einer mehr oder weniger unbeständigen restlichen Komponente, welche den einzelnen Gliedern zugeeignet ist und bald negativ bald positiv ausfällt, angesehen werden. Graphisch ließe sich also eine evolutorische Reihe etwa folgendermaßen darstellen:

Fig. 1.

AB würde die "glatte" und *MN* die unbeständige "restliche" Komponente bedeuten, die man vielleicht auch "episäkulare Komponente" nennen könnte.

Entsprechend den Eigenschaften des zu lösenden konkreten statistischen Problems, kann sich unser Interesse einmal dem Studium der Reihe *AB*, ein anderes Mal der Analyse der Reihe *MN* zuwenden. Ist letzteres der Fall, so werden die angewandten Methoden (darunter auch die "Variate-Difference"-Methode) die Reihe *MN*, oder eine bestimmte Funktion derselben, von der Reihe *AB* abzusondern suchen, indem angenommen wird, daß *MN* genau dieselben Schwankungen auch um ein Konstantes (also in Diagramm horizontales) *AB* aufweisen würde. Ist die "restliche" Komponente auf irgendeine Art isoliert und ist das so geschehen, daß die Summe der negativen Reihenglieder derjenigen der positiven gleich ist, so kann sich folgendes ergeben:

(1) Einem positiven Gliede der Reihe folgt öfter ein negatives, als ein positives, Glied auf dem Fuße, und einem negativen—eher ein positives, als umgekehrt. Die Reihe wird uns "zackig" erscheinen.

(2) Einem positiven Gliede folgt ebenso oft ein negatives, als ein positives, Glied nach, und einem negativen—ebenso oft ein positives, als umgekehrt. Auch diese Reihe hat ein "zackiges" Aussehen, jedoch weniger, als es bei der in Nr. 1 geschilderten der Fall ist.

* Die vorliegende Schrift ist ein Auszug aus einer ungedruckten größeren Arbeit des Verfassers über die "Variate-Difference"-Methode (in russischer Sprache).

(3) Einem positiven Gliede folgt öfter ein ebenfalls positives und einem negativen—öfter ein ebenfalls negatives nach, als umgekehrt. Eine solche Reihe wird uns *glatter* erscheinen, als die beiden vorhergehenden.

Die Unterscheidung dieser 3 Typen diene zum besseren Verständnis der folgenden Klassifikation, welche, obgleich sie auf einem ganz anderen *principium divisionis* beruht, mit ersterer in Vielem übereinstimmt.

Es können nämlich, je nach ihrem Verhalten beim endlichen Differenzieren, vier Gruppen "episäkularer" Reihen unterschieden werden:

(1) *Gruppe "Z"* (vom Worte "zackig"). Außer Yule's wohlbekannten kurzperiodischen Sinus-Reihen, gehören zu dieser Gruppe auch einige andere Typen, deren gemeinsames (aber an und für sich *nicht genügendes*) Merkmal eine negative Korrelation zwischen den einander unmittelbar nachfolgenden Reihengliedern sein wird.

(2) *Gruppe "R"* (vom Worte "random"). Zu dieser Gruppe gehören nicht nur in meinen Sinne* "oszillatorische" Reihen, welche als empirische Größen, die eine Variable mit einen konstanten Verteilungsgesetz bei von einander unabhängigen Versuchen annimmt, definiert werden können, sondern auch Prof. Al. A. Tchouproff's "uniforme"† und noch einige andere Reihen, deren Charakteristik zuweilen recht kompliziert ausfallen kann ‡.

(3) *Gruppe "G"* (vom Worte "glatt"). Zu ihr gehören nicht nur parabolische Reihen höherer Ordnung und Sinus-Reihen mit einer positiven Korrelation der Nachbarglieder, sondern auch eine Reihe anderer Typen, deren gemeinsames Merkmal, welches aber an und für sich nicht genügt, ebenfalls eine positive Korrelation der Nachbarglieder sein wird.

Die einzelnen Glieder der Reihen vom Typus "*G*" wachsen beim endlichen Differenzieren *langsamer* und die einzelnen Glieder der Reihen vom Typus "*Z*" *rascher*, als es beim Typus "*R*" der Fall ist, welch letzterer sich beim endlichen Differenzieren "oszillatorisch" beninmt.

(4) *Gruppe "S"* (vom Worte "Summa"). Zu dieser Gruppe gehören Reihen, welche beim endlichen Differenzieren sich anfänglich wie zu einer der drei vorhergehenden Gruppen gehörig benehmen um später bei höheren Differenzen in andere Gruppen überzugehen. Man kann sie als algebraische Summen von Reihen, die zu verschiedenen Typen gehören, betrachten.

Die mathematische Untersuchung dieser vier Gruppen ist recht umständlich und muß daher hier übergangen werden.

Die "Variate-Difference"-Methode ist auf die Herausschälung der "episäkularen" Reihen vom Typus "*R*" eingestellt, und es ist klar, daß wenn es sich erweisen sollte, daß solche Reihen in der statistischen Praxis überhaupt nicht vorkommen, die ganze Methode als gegenstandslos zusammenbrechen würde. Des-

* Vergl. *Biometrika*, Vol. x, pp. 269—279.

† Al. A. Tchouproff, "On the mathematical expectation of the moments of frequency distributions in the case of correlated observations," *Metron*, Vol. II.

‡ Vergl. Anmerkung zu Seite 146.

gleichen würde sie einen schweren (wenn auch vielleicht nicht tödlichen) Schlag erleiden, wenn es sich herausstellen sollte, daß alle "episäkularen" Reihen zum Typus "S" gehören und eine starke "Z"-Komponente aufweisen.

Deshalb ist es, zusammen mit Prof. K. Pearson, anzunehmen, daß das Schicksal der "Variate-Difference"-Methode jetzt in erster Linie von den Resultaten konkreter statistischer Untersuchungen abhängt. Gelingt es, das mehr oder minder häufige Vorkommen des Typus "R" nachzuweisen, ist die Methode gerechtfertigt. Ist das entgegengesetzte der Fall, so wird sie wohl aus dem Waffen-Arsenal des Statistikers scheiden müssen. Derartige konkrete statistische Untersuchungen einzelner Reihen und Reihenpaare besitzen aber auch ein selbständiges theoretisches Interesse, da sie in den Fragenkomplex der "Stabilität statistischer Reihen" hineinspielen.

Den Gegenstand der vorliegenden Schrift bildet die Darstellung eines Verfahrens, welches die Arbeit, die die Anwendung der "Variate-Difference"-Methode erfordert, um einiges erleichtert, und auch dem verallgemeinerten Verfahren vorarbeitet, welches Prof. K. Pearson und E. M. Elderton neulich angegeben haben*.

Es seien zwei statistische Reihen: $x_1, x_2, x_3, \ldots x_n$ und $y_1, y_2, y_3, \ldots y_n$ gegeben und es sei E das Zeichen für mathematische Erwartung; ferner sei angenommen, daß für ein gewisses k, welches $< n$ ist, das Gleichungsystem besteht:

$$\frac{E\sum_{i=1}^{n-k-j} x_i y_{i+j}}{n-k-j} = \frac{E\sum_{i=1}^{n-k-j} x_{i+1} y_{i+j+1}}{n-k-j} = \ldots = \frac{E\sum_{i=1}^{n-k-j} x_{i+k} y_{i+j+k}}{n-k-j} = \frac{E\sum_{i=1}^{n-j} x_i x_{i+j}}{n-j} \quad \ldots\ldots (1),$$

wenn $j = 0, 1, 2, 3, \ldots$. Je größer n und je geringer k im Vergleich zu n, desto genauer trifft diese Annahme für eine jede Reihe zu. Setzt man noch

$$r_j = \frac{E\sum_{i=1}^{n-j} x_i y_{i+j}}{n-j} \Bigg/ \sqrt{\frac{E\sum_{i=1}^{n} x_i^2}{n} \cdot \frac{E\sum_{i=1}^{n} y_i^2}{n}} = \frac{E\sum_{i=1}^{n-j} x_i y_{i+j}}{(n-j)\sigma_x \sigma_y} \quad \ldots\ldots\ldots\ldots (2)$$

(x und y werden ein jedes als Abweichungen vom entsprechenden in *mathemat. Erwart.* ausgedrückten Mittelwert gedacht), so erhält man nach einigen Umformungen:

$$\frac{E\sum_{i=1}^{n-k-j} \Delta^k x_i \Delta^k y_{i+j}}{n-k-j} = \sigma_x \sigma_y (-1)^k \left\{1 \,.\, r_{k+j} - \frac{2k}{1} r_{k+j-1} + \frac{2k(2k-1)}{1\,.\,2} r_{k+j-2} - \ldots + 1 \,.\, r_{j-k}\right\}^{\dagger}.$$

Denkt man sich nun die Reihe:

$$r_{k+j}, r_{k+j-1}, r_{k+j-2}, \ldots r_j, r_{j-1}, \ldots r_{j-k}, r_{j-k-1} \ldots\ldots\ldots\ldots\ldots (3),$$

* Karl Pearson and Ethel M. Elderton, "On the Variate Difference Method," *Biometrika*, Vol. XIV, p. 294 ff.

† Hier, wie überall in dieser Schrift, werden nicht "central differences" genommen sondern folgende Bezeichnungen gebraucht:

$$\Delta' x_i = x_i - x_{i+1};\ \Delta'' x_i = \Delta' x_i - \Delta' x_{i+1} = x_i - 2x_{i+1} + x_{i+2};\ \ldots\ \Delta^k r_i = x_i - k x_{i+1} + \frac{k\,.\,(k-1)}{1\,.\,2} x_{i+2} - \ldots,\ \text{u.s.w.}$$

so ist es klar daß der Ausdruck in den Klammern { } gleich $\Delta^{2k} r_{k+j}$, der $(2k)$-ten Differenz von r_{k+j} in der Reihe (3) sein muß.

Folglich ist:

$$\frac{E \sum_{i=1}^{n-k-j} \Delta^k x_i \, \Delta^k y_{i+j}}{n-k-j} = \sigma_x \sigma_y (-1)^k \Delta^{2k} r_{k+j} \quad \ldots\ldots\ldots\ldots (4),$$

und daraus:

$$\frac{E \sum_{i=1}^{n-k} \Delta^k x_i \Delta^k y_i}{n-k} = \sigma_x \sigma_y (-1)^k \Delta^{2k} r_k \quad \ldots\ldots\ldots\ldots (5).$$

Setzt man nun in (4) x_{i+j}, statt y_{i+j}, und

$$r_j' = \frac{E \sum_{i=1}^{n-j} x_i x_{i+j}}{n-j} \Bigg/ \frac{E \sum_{i=1}^{n} x_i^2}{n} \quad \ldots\ldots\ldots\ldots (6),$$

so erhält man ferner nach entsprechender Umformung der Bedingungsgleichungen (1) (wo ebenfalls überall x, statt y, mit denselben Indexen zu setzen ist):

$$\frac{E \sum_{i=1}^{n-k-j} \Delta^k x_i \, \Delta^k x_{i+j}}{n-k-j} = \sigma_x^2 (-1)^k \Delta^{2k} r'_{k+j} \ldots\ldots\ldots\ldots (7)$$

und

$$\frac{E \sum_{i=1}^{n-k} \Delta^k x_i^2}{n-k} = \sigma_x^2 (-1)^k \Delta^{2k} r_k' \quad \ldots\ldots\ldots\ldots (7\,a).$$

(Vergl. K. Pearson and E. M. Elderton, loc. cit. Form. XVIII. und XXI.)

Es ist dabei noch zu berücksichtigen, daß bei solchen Bezeichnungen für eine *gegebene* x-Reihe rein algebraisch: $r_j' = r'_{-j}$, da bei positivem j:

$$\sum_{i=1}^{n-j} x_i x_{i+j} = (x_1 x_{1+j} + x_2 x_{2+j} + \ldots + x_{n-j} x_n) = \sum_{i=1}^{n-j} x_{i+j} x_i.$$

Dieser Satz gilt aber *nicht* für r_j und r_{-j}, die einander nicht gleich zu sein brauchen. Außerdem ist offenbar $r_0' = 1$.

Für eine in meinem Sinne "*oszillatorische*" Reihe besteht die Gleichung:

$$\frac{E \sum_{i=1}^{n-k-1} \Delta^{k+1} x_i \Delta^{k+1} y_i}{\frac{(2k+2)!}{(k+1)!\,(k+1)!}(n-k-1)} = \frac{E \sum_{i=1}^{n-k} \Delta^k x_i \Delta^k y_i}{\frac{2k!}{k!\,k!}(n-k)}.$$

Bei Anwendung von Form. (4) können wir jetzt schreiben:

$$0 = \frac{\sigma_x \sigma_y (-1)^{k+1} \Delta^{2k+2} r_{k+1}}{\frac{(2k+2)!}{(k+1)!\,(k+1)!}} - \frac{\sigma_x \sigma_y (-1)^k \Delta^{2k} r_k}{\frac{2k!}{k!\,k!}}$$

$$= \frac{\sigma_x \sigma_y (-1)^k}{\frac{2k!}{k!\,k!}} \left[- \frac{\Delta^{2k} r_{k+1} - 2\Delta^{2k} r_k + \Delta^{2k} r_{k-1}}{\frac{2\,(2k+1)}{k+1}} - \Delta^{2k} r_k \right]$$

$$= \frac{\sigma_x \sigma_y (-1)^k \Delta^{2k} r_k}{\frac{(2k+1)!}{(k+1)!\,k!}} \left[- \tfrac{1}{2} \left(\frac{\Delta^{2k} r_{k+1}}{\Delta^{2k} r_k} + \frac{\Delta^{2k} r_{k-1}}{\Delta^{2k} r_k} \right) - \frac{k}{k+1} \right].$$

Nun ist aber

$$\frac{\Delta^{2k} r_{k+1}}{\Delta^{2k} r_k} = \frac{\sigma_x \sigma_y (-1)^k \Delta^{2k} r_{k+1}}{\sigma_x \sigma_y (-1)^k \Delta^{2k} r_k} = \frac{E \sum_{i=1}^{n-k-1} \Delta^k x_i \Delta^k y_{i+1}}{n-k-1} \Big/ \frac{E \sum_{i=1}^{n-k} \Delta^k x_i \Delta^k y_i}{n-k}.$$

Bezeichnet man diese Größe durch ${}_k r_1$ und entsprechend:

$$\frac{E \sum_{i=1}^{n-k-1} \Delta^k x_{i+1} \Delta^k y_i}{n-k-j} \Big/ \frac{E \sum_{i=1}^{n-k} \Delta^k x_i \Delta^k y_i}{n-k}$$

durch ${}_k r_{-1}$, so ergiebt sich daraus:

$$\frac{E \sum_{i=1}^{n-k-1} \Delta^{k+1} x_i \Delta^{k+1} y_i}{\frac{(2k+2)!}{(k+1)!\,(k+1)!}(n-k-1)} - \frac{E \sum_{i=1}^{n-k} \Delta^k x_i \Delta^k y_i}{\frac{2k!}{k!\,k!}(n-k)}$$

$$= \frac{\sigma_x \sigma_y (-1)^k \Delta^{2k} r_k}{\frac{(2k+1)!}{(k+1)!\,k!}} \left[-\frac{{}_k r_1 + {}_k r_{-1}}{2} - \frac{k}{k+1} \right] = 0 \quad \ldots\ldots(8),$$

und folglich

$$\frac{{}_k r_1 + {}_k r_{-1}}{2} = -\frac{k}{k+1}.$$

Desgleichen erhalten wir, wenn wir

$$\frac{E \sum_{i=1}^{n-k-1} \Delta^k x_i \Delta^k x_{i+1}}{n-k-1} \Big/ \frac{E \sum_{i=1}^{n-k} \Delta^k x_i^2}{n-k} = {}_k r_1' \text{ setzen}: \quad \ldots\ldots\ldots\ldots(9),$$

$$\frac{E \sum_{i=1}^{n-k-1} \Delta^{k+1} x_i^2}{\frac{(2k+2)!}{(k+1)!\,(k+1)!}(n-k-1)} - \frac{E \sum_{i=1}^{n-k} \Delta^k x_i^2}{\frac{2k!}{k!\,k!}(n-k)} = \frac{\sigma_x^2 (-1)^k \Delta^{2k} r_k'}{\frac{(2k+1)!}{(k+1)!\,k!}} \left[-{}_k r_1' - \frac{k}{k+1} \right] = 0$$

$$\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(10),$$

und daraus:

$${}_k r_1' = -\frac{k}{k+1}.$$

Bekanntlich ist aber $-\frac{k}{k+1}$ gerade diejenige Größe, welche ${}_k r_1'$ für eine "oszillatorische" Reihe annehmen muß. Somit ist es erwiesen daß

$$\frac{E \sum_{i=1}^{n-k-1} \Delta^{k+1} x_i \Delta^{k+1} y_i}{n-k-1} \text{ sich zu } \frac{E \sum_{i=1}^{n-k} \Delta^k x_i \Delta^k y_i}{n-k} \text{ und } \frac{E \sum_{i=1}^{n-k-1} \Delta^{k+1} x_i^2}{n-k-1} \text{ sich zu } \frac{E \sum_{i=1}^{n-k} x_i^2}{n-k}$$

nur in dem Falle so verhalten werden, wie es bei "oszillatorischen" Reihen sein muß, wenn $\frac{{}_k r_1 + {}_k r_{-1}}{2} = -\frac{k}{k+1} = {}_k r_1'$, d. h. wenn letztere Größen sich ebenfalls "oszillatorisch" verhalten. Diesen Satz kann man auch umkehren.

Für den allgem. Fall untersuchen wir den Ausdruck:

$$\frac{E\sum_{i=1}^{n-k-m}\Delta^{k+m}x_i\Delta^{k+m}y_i}{\frac{(2k+2m)!}{(k+m)!\,(k+m)!}(n-k-m)}-\frac{E\sum_{i=1}^{n-k}\Delta^k x_i\Delta^k y_i}{\frac{2k!}{k!\,k!}(n-k)}$$

oder, was dasselbe ist:

$$\frac{\sigma_x\sigma_y(-1)^{k+m}\Delta^{2k+2m}r_{k+m}}{\frac{(2k+2m)!}{(k+m)!\,(k+m)!}}-\frac{\sigma_x\sigma_y(-1)^k\Delta^{2k}r_k}{\frac{2k!}{k!\,k!}}.$$

Da nun allgemein:

$$\Delta^{2k+2m}x_i=\Delta^{2m}(\Delta^{2k}x_i)=\Delta^{2k}x_i-2m\Delta^{2k}x_{i+1}+\frac{2m(2m-1)}{1.2}\Delta^{2k}x_{i+2}-\cdots,$$

so erhält man daraus:

$$\frac{\sigma_x\sigma_y(-1)^k}{\frac{2k!}{k!\,k!}}\left\{\frac{(-1)^m\left[\Delta^{2k}r_{k+m}-\frac{2m}{1}\Delta^{2k}r_{k+m-1}+\frac{2m(2m-1)}{1.2}\Delta^{2k}r_{k+m-2}-\cdots\right]}{\frac{(2k+2m)!}{(k+m)!\,(k+m)!}\cdot\frac{k!\,k!}{2k!}}-\Delta^{2k}r_k\right\}.$$

Setzt man jetzt

$$\frac{\Delta^{2k}r_{k+j}}{\Delta^{2k}r_k}=\frac{\sigma_x\sigma_y(-1)^k\Delta^{2k}r_{k+j}}{\sigma_x\sigma_y(-1)^k\Delta^{2k}r_k}=\frac{E\sum_{i=1}^{n-k-j}\Delta^k x_i\Delta^k x_{i+j}}{n-k-j}\Bigg/\frac{E\sum_{i=1}^{n-k}\Delta^k x_i\Delta^k y_i}{n-k}={}^kr_j\quad\ldots(11),$$

so verwandelt sich unser Ausdruck in:

$$\frac{\sigma_x\sigma_y(-1)^k\Delta^{2k}r_k}{\frac{(2k+2m)!}{(k+m)!\,(k+m)!}}\left\{(-1)^m\Delta^{2m}{}^kr_m-\frac{(2\kappa+2m)!\,k!\,k!}{(k+m)!\,(k+m)!\,2k!}\right\}\quad\ldots\ldots(12).$$

Für eine "oszillatorische" Reihe ist bekanntlich:

$${}^kr_1'=-\frac{k}{k+1},\quad {}^kr_2'=+\frac{k(k-1)}{(k+1)(k+2)};\quad\ldots,\ {}^kr_j'=(-1)^j\frac{k!\,k!}{(k-j)!\,(k+j)!}=(-1)^j\frac{C_{2k}^{k+j}}{C_{2k}^k}$$

$$\ldots\ldots\ldots\ldots\ldots\ldots(13).$$

Nimmt man nun an:

$$\frac{{}^kr_1+{}^kr_{-1}}{2}=-\frac{k}{k+1},\quad\frac{{}^kr_2+{}^kr_{-2}}{2}=+\frac{k(k-1)}{(k+1)(k+2)};\quad\ldots\frac{{}^kr_j+{}^kr_{-j}}{2}=(-1)^j\frac{k!\,k!}{(k-j)!\,(k+j)!},$$

und setzt diese Werte in (12), so ergiebt sich nach einigen Umformungen:

$$\frac{\sigma_x\sigma_y(-1)^k\Delta^{2k}r_k}{\frac{(2k+2m)!}{(k+m)!\,(k+m)!}}\left\{\frac{(2k+2m)!\,k!\,k!}{(k+m)!\,(k+m)!\,2k!}-\frac{(2k+2m)!\,k!\,k!}{(k+m)!\,(k+m)!\,2k!}\right\}.$$

In diesem Falle verwandelt sich also (12) in Null.

Desgleichen wird auch

$$\frac{E\sum_{i=1}^{n-k-m}\Delta^k x_i^2}{\frac{(2k+2m)!}{(k+m)!\,(k+m)!}(n-k-m)}-\frac{E\sum_{i=1}^{n-k}\Delta^k x_i^2}{\frac{2k!}{k!\,k!}(n-k)}$$

gleich Null, wenn alle ${}_k r_i'$, von ${}_k r_1'$ bis ${}_k r_m'$, die einer "oszillatorischen" Reihe zukommenden Größen annehmen.

Man kann also folgende Regel aufstellen: ist für die k-te Differenz einer statistischen Reihe

$${}_k r_1' = \frac{E\sum_{i=1}^{n-k-1} \Delta^k x_i \Delta^k x_{i+1}}{n-k-1} \Big/ \frac{E\sum_{i=1}^{n-k} \Delta^k x_i^2}{n-k} = -\frac{k}{k+1},$$

so wird auch $\left(4 - \frac{2}{k+1}\right) \frac{E\sum_{i=1}^{n-k} \Delta^k x_i^2}{n-k}$ gleich $\frac{E\sum_{i=1}^{n-k-1} \Delta^{k+1} x_i^2}{n-k-1}$

sein; ist *außerdem* auch ${}_k r_2' = \frac{+k(k-1)}{(k+1)(k+2)}$, so wird desgleichen auch

$$\left(4 - \frac{2}{k+2}\right) \frac{E\sum_{i=1}^{n-k-1} \Delta^{k+1} x_i^2}{n-k-1} \text{ gleich } \frac{E\sum_{i=1}^{n-k-2} \Delta^{k+2} x_i^2}{n-k-2}$$

sein müssen.

Kann man dasselbe Verfahren *sukzessive* bis zur Konstatierung der Gleichung

$${}_k r_j' = \frac{E\sum_{i=1}^{n-k-j} \Delta^k x_i \Delta^k x_{i+j}}{n-k-j} \Big/ \frac{E\sum_{i=1}^{n-k} \Delta^k x_i^2}{n-k} = (-1)^j \frac{k!\,k!}{(k+j)!\,(k-j)!}$$

fortsetzen, so wird auch die Gleichung:

$$\left(4 - \frac{2}{k+j}\right) \frac{E\sum_{i=1}^{n-k-j+1} \Delta^{k+j-1} x_i^2}{n-k-j+1} = \frac{E\sum_{i=1}^{n-k-j} \Delta^{k+j} x_i^2}{n-k-j}$$

unbedingt stattfinden müssen.

Dieselbe Regel ist auch für die Größen ${}_k r_1$, ${}_k r_2$ ... u. ff. giltig; man muß aber hier

$$\frac{{}_k r_1 + {}_k r_{-1}}{2} = -\frac{k}{k+1}, \ldots \frac{{}_k r_j + {}_k r_{-j}}{2} = (-)^j \frac{k!\,k!}{(k-j)!\,(k+j)!}$$

setzen können.

Es ist ersichtlich, daß wenn die Größen ${}_k r_1'$, ${}_k r_2'$, ${}_k r_3'$ ... durch empirische Annäherungswerte ersetzt werden, dieselben alle denselben Nenner besitzen und folglich bei größerem k leichter zu berechnen sein werden, als die *stand. deviat.* für die $(k+1)$-te, $(k+2)$-te, $(k+3)$-te u.s.w. Differenzen. Denn die schwierigste und am leichtesten zu Rechenfehlern führende Arbeit bei Anwendung der "Variate-Difference"-Methode ist eben die Berechnung der höheren Differenzen der Grundreihe. Dasselbe gilt auch für die Größen ${}_k r_1$, ${}_k r_2$, ${}_k r_3$, ... und die entsprechenden Korrelationskoeffizienten für die $(k+1)$-te, $(k+2)$-te u.s.w. Differenzen. Es müssen nur ein für die Praxis bequemes Kriteriensystem aufgebaut und die entsprechenden Fehlergrenzen bestimmt werden.

Setzt man in der linken Seite von (8) überall $(k+m)$ statt k, und in der rechten Δ_1, statt 0, so erhält man:

$$\Delta_1 = \frac{E\sum_{i=1}^{n-k-m-1}\Delta^{k+m+1}x_i\,\Delta^{k+m+1}y_i}{\frac{(2k+2m+2)!}{(k+m+1)!\,(k+m+1)!}(n-k-m-1)} - \frac{E\sum_{i=1}^{n-k-m}\Delta^{k+m}x_i\,\Delta^{k+m}y_i}{\frac{(2k+2m)!}{(k+m)!\,(k+m)!}(n-k-m)}$$

$$= \frac{\sigma_x\sigma_y(-1)^{k+m}\,\Delta^{2k+2m}r_{k+m}}{\frac{(2k+2m+1)!}{(k+m+1)!\,(k+m)!}}\left[-\frac{{}_{k+m}r_1+{}_{k+m}r_{-1}}{2}-\frac{k+m}{k+m+1}\right] \quad \ldots(14).$$

Nun ist aber nach (11):

$${}_{k+m}r_1 = \frac{\Delta^{2k+2m}r_{k+m+1}}{\Delta^{2k+2m}r_{k+m}} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(14\,a).$$

Andererseits aber:

$$\frac{\Delta^{2k+2m}r_{k+m+1}}{\Delta^{2k+2m}r_{k+m}} = \frac{\Delta^{2m}(\Delta^{2k}r_{k+m+1})}{\Delta^{2m}(\Delta^{2k}r_{k+m})}$$

$$= \frac{\Delta^{2k}r_{k+m+1} - 2m\Delta^{2k}r_{k+m} + \frac{2m(2m-1)}{1.2}\Delta^{2k}r_{k+m-1} - \ldots}{\Delta^{2k}r_{k+m} - 2m\Delta^{2k}r_{k+m-1} + \frac{2m(2m-1)}{1.2}\Delta^{2k}r_{k+m-2} - \ldots}.$$

Dividiert man hier Zähler und Nenner durch $\Delta^{2k}r_k$, so ergiebt sich daraus:

$${}_{k+m}r_1 = \frac{\Delta^{2m}{}_kr_{m+1}}{\Delta^{2m}{}_kr_m} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(14\,b).$$

Setzt man diese Größen und die entsprechenden für ${}_{k+m}r_{-1}$ in (14) ein, so erhält man zwei Formeln:

$$\Delta_1 = \frac{\sigma_x\sigma_y(-1)^{k+m}\,\Delta^{2k+2m}r_{k+m}}{\frac{(2k+2m+1)!}{(k+m+1)!\,(k+m)!}}\left[-\frac{\Delta^{2k+2m}r_{k+m+1}+\Delta^{2k+2m}r_{k+m-1}}{2\Delta^{2k+2m}r_{k+m}}-\frac{k+m}{k+m+1}\right]$$

$$\ldots\ldots\ldots\ldots\ldots\ldots(15\,a),$$

$$\Delta_1 = \frac{\sigma_x\sigma_y(-1)^{k+m}\,\Delta^{2k+2m}r_{k+m}}{\frac{(2k+2m+1)!}{(k+m+1)!\,(k+m)!}}\left[-\frac{\Delta^{2m}{}_kr_{m+1}+\Delta^{2m}{}_kr_{m-1}}{2\Delta^{2m}{}_kr_m}-\frac{k+m}{k+m+1}\right] \quad (15\,b).$$

Auf dieselbe Art erhalten wir auch:

$$\Delta_2 = \frac{E\sum_{i=1}^{n-k-m-1}\Delta^{k+m+1}x_i^2}{\frac{(2k+2m+2)!}{(k+m+1)!\,(k+m+1)!}(n-k-m-1)} - \frac{E\sum_{i=1}^{n-k-m}\Delta^{k+m}x_i^2}{\frac{(2k+2m)!}{(k+m)!\,(k+m)!}(n-k-m)}$$

$$= \frac{\sigma_x^2(-1)^{k+m}\,\Delta^{2k+2m}r'_{k+m}}{\frac{(2k+2m+1)!}{(k+m+1)!\,(k+m)!}}\left[-{}_{k+m}r_1'-\frac{k+m}{k+m+1}\right] \quad \ldots\ldots\ldots(16),$$

$$\Delta_2 = \frac{\sigma_x^2(-1)^{k+m}\,\Delta^{2k+2m}r'_{k+m}}{\frac{(2k+2m+1)!}{(k+m+1)!\,(k+m)!}}\left[-\frac{\Delta^{2k+2m}r'_{k+m+1}}{\Delta^{2k+2m}r'_{k+m}}-\frac{k+m}{k+m+1}\right] \quad \ldots\ldots\ldots(16\,a),$$

$$\Delta_2 = \frac{\sigma_x^2(-1)^{k+m}\,\Delta^{2k+2m}r'_{k+m}}{\frac{(2k+2m+1)!}{(k+m+1)!\,(k+m)!}}\left[-\frac{\Delta^{2m}{}_kr'_{m+1}}{\Delta^{2m}{}_kr'_m}-\frac{k+m}{k+m+1}\right] \quad \ldots\ldots\ldots\ldots(16\,b).$$

Formeln 15 *a*, 15 *b* und 16 *a*, 16 *b* sind die Grundformeln, auf denen das neue Verfahren beruht. Wir beschränken uns hier auf die Analyse der letzteren zwei, da sie kürzer zu fassen ist. Die entsprechenden Änderungen für Formeln 15 *a* und 15 *b* sind nicht schwer durchzuführen. Desgleichen übergehen wir auch die Zusammenstellung von Δ_1 und Δ_2, die für die Verallgemeinerung der "Variate-Difference"-Methode von einiger Bedeutung zu sein scheint.

Da nach (7 *a*):

$$\sigma_x^2 (-1)^{k+m} \Delta^{2k+2m} r'_{k+m} = \frac{E \sum_{i=1}^{n-k-m} \Delta^{k+m} x_i^2}{n - k - m},$$

und also immer positiv bleibt, so wird das Zeichen von (16 *a*) und (16 *b*) durch den Ausdruck

$$D = \left[- \frac{\Delta^{2k+2m} r'_{k+m+1}}{\Delta^{2k+2m} r'_{k+m}} - \frac{k+m}{k+m+1} \right] = \left[- \frac{\Delta^{2m}{}_k r'_{m+1}}{\Delta^{2m}{}_k r'_m} - \frac{k+m}{k+m+1} \right] \quad \ldots(17)$$

bestimmt. Ist $D = 0$, so wird die *stand. deviation* der $(k+m+1)$-ten Differenz sich zu der *stand. deviation* der $(k+m)$-ten Differenz so verhalten, wie es einer "oszillatorischen" Reihe zukommt. Ist $D > 0$, so wächst sie *rascher*, als es bei einer "*R*"-Reihe der Fall ist, und folglich gehört die "episäkulare" Reihe zum reinen "*Z*"- oder aber zum "*S*"-Typus mit einer "*Z*"-Komponente. Ist $D < 0$, so ist die "glatte" Komponente noch nicht restlos eliminiert, oder aber besitzt die "episäkulare" Reihe ein starkes "*G*"-Element.

Es ist daher einleuchtend, daß *D* ein ganz brauchbares Kriterium für die Anwendung der "Variate-Difference"-Methode abgeben könnte, vorausgesetzt es aus empirischen Daten bestimmbar ist, für verschiedene Differenzen den gleichen Grenzwert behält und sein Fehlerquadrat nicht zu groß ist.

Aus (16) ist unmittelbar ersichtlich, daß der maximale *positive* Betrag von *D* nicht $+1 - \frac{k+m}{k+m+1} = \frac{1}{k+m+1}$ übersteigen kann, während seine *negative* Grenze $-\frac{2k+2m+1}{k+m+1}$ ausmacht. Nun besitzt ein positives *D* für uns ein spezielles Interesse, da es die Anwesenheit derjenigen Komponente angiebt, die für die Anwendung der ganzen "Variate-Difference"-Methode von so fataler Bedeutung ist.

Es liegt der Gedanke nahe, *D* den Multiplikator $(k+m+1)$ vorzusetzen, was seine maximale Größe gleich $+1$ und *D* zu einer Art Intensitätsmesser des Einflusses der "*Z*"-Komponente erheben würde. Wir bestimmen daher aus (16 *a*) und (16 *b*):

$${}_0D_{k+m+1} = - \frac{(k+m+1)\,\Delta^{2k+2m} r'_{k+m+1}}{\Delta^{2k+2m} r'_{k+m}} - (k+m) \quad \ldots\ldots(17\,a),$$

$${}_kD_{m+1} = - \frac{(k+m+1)\,\Delta^{2m}{}_k r'_{m+1}}{\Delta^{2m}{}_k r'_m} - (k+m) \quad \ldots\ldots(17\,b).$$

Die *negative* Grenze der Ausdrücke (17 *a*) und (17 *b*), *die einander gleich sein müssen*, beträgt $-(2k+2m+1)$, wächst also mit zunehmendem *k* und *m*.

Setzt man nun in (17 *a*) die Größe $(k+m)$ sukzessive gleich 0, 1, 2, 3, 4, ... und in (17 *b*) m sukzessive gleich 0, 1, 2, 3, 4, ..., so erhält man zwei für den praktischen Gebrauch geeignete Systeme:

$$
\left.
\begin{aligned}
{}_0D_1 &= -r_1', \\
{}_0D_2 &= \frac{-r_1'+r_2'}{1-r_1'}, \\
{}_0D_3 &= \frac{-5r_1'+8r_2'-3r_3'}{6-8r_1'+2r_2'}, \\
{}_0D_4 &= \frac{-7r_1'+14r_2'-9r_3'+2r_4'}{10-15r_1'+6r_2'-r_3'}, \\
{}_0D_5 &= \frac{-42r_1'+96r_2'-81r_3'+32r_4'-5r_5'}{70-112r_1'+56r_2'-16r_3'+2r_4'}, \\
{}_0D_6 &= \frac{-66r_1'+165r_2'-165r_3'+88r_4'-25r_5'+3r_6'}{126-210r_1'+120r_2'-45r_3'+10r_4'-r_5'}, \\
{}_0D_7 &= \frac{-429r_1'+1144r_2'-1287r_3'+832r_4'-325r_5'+72r_6'-7r_7'}{924-1584r_1'+990r_2'-440r_3'+132r_4'-24r_5'+2r_6'}, \quad \text{u.s.w.}
\end{aligned}
\right\} \ldots(18\,a),
$$

$$
\left.
\begin{aligned}
{}_kD_1 &= -(k+1)\,{}_kr_1' - k, \\
{}_kD_2 &= \frac{-2\,{}_kr_1'+(k+2)\,{}_kr_2'-k}{2-2\,{}_kr_1'}, \\
{}_kD_3 &= \frac{+(k-5)\,{}_kr_1'+(2k+8)\,{}_kr_2'-(k+3)\,{}_kr_3'-2k}{6-8\,{}_kr_1'+2\,{}_kr_2'}, \\
{}_kD_4 &= \frac{+(4k-14)\,{}_kr_1'+(4k+28)\,{}_kr_2'-(4k+18)\,{}_kr_3'+(k+4)\,{}_kr_4'-5k}{20-30\,{}_kr_1'+12\,{}_kr_2'-2\,{}_kr_3'}, \\
{}_kD_5 &= \frac{+(14k-42)\,{}_kr_1'+(8k+96)\,{}_kr_2'-(13k+81)\,{}_kr_3'+(6k+32)\,{}_kr_4'-(k+5)\,{}_kr_5'-14k}{70-112\,{}_kr_1'+56\,{}_kr_2-16\,{}_kr_3'+2\,{}_kr_4'},
\end{aligned}
\right\}
$$

u.s.w. .....(18 *b*).

Die Gleichungen (18 *a*) können auch direkt aus (18 *b*) erhalten werden, indem man hier $k=0$ setzt.

In allen für die Praxis in Betracht kommenden Fällen bleiben die Größen r_1', r_2', r_3', ... ${}_kr_1'$, ${}_kr_2'$, ${}_kr_3'$ ... unbekannt, da sie Quotienten von mathematischen Erwartungen darstellen. Setzt man aber in die Formeln (18 *a*) und (18 *b*) die entsprechenden empirischen Ausdrücke, also etwa:

$$
\rho_j' = \frac{\sum_{i=1}^{n-j} x_i' x'_{i+j} - (n-j)\,x_{(n)}'^2}{n-j} \Bigg/ \frac{\sum_{i=1}^{n} x_i'^2 - n x_{(n)}'^2}{n-1}
$$

für r_j' und

$$
{}_k\rho_j' = \frac{\sum_{i=1}^{n-k-j} \Delta^k x_i' \, \Delta^k x'_{i+j}}{n-k-j} \Bigg/ \frac{\sum_{i=1}^{n-k} \Delta^k x_i'^2}{n-k}
$$

für ${}_kr_j'$, wobei x_1', x_2', ... x_n' jetzt die Glieder der konkreten statistischen Reihe

(und nicht Abweichungen vom in mathem. Erwartungen ausgedrückten Mittelwerte) bedeuten und $x_{(n)}$ für $\frac{1}{n}\overset{n}{\underset{1}{\Sigma}} x_i'$ steht*,—so muß man sich für den dabei zu begehenden Fehler Rechenschaft ablegen können.

Wäre die Umformung (7 *a*) genau und bestände ferner eine algebraische Identität

$$\frac{\overset{n-k}{\underset{i=1}{\Sigma}} \Delta^k x_i^2}{n-k} = \frac{\overset{n}{\underset{i=1}{\Sigma}} (x_i' - x_{(n)})^2}{n-1} \cdot (-1)^k \Delta^{2k} \rho_k',$$

so würde die Aufgabe auf die Bestimmung von

$$E\left\{\frac{\overset{n-k-1}{\underset{i=1}{\Sigma}} \Delta^{k+1} x_i^2}{\frac{(2k+2)!}{(k+1)!\,(k+1)!}(n-k-1)} - \frac{\overset{n-k}{\underset{i=1}{\Sigma}} \Delta^k x_i^2}{\frac{2k!}{k!\,k!}(n-k)}\right\}^2 \text{ hinauslaufen ...(19).}$$

Leider ist aber obige Gleichung nur bei Annahme der Bedingungen:

$$\frac{\overset{n-k-j}{\underset{i=1}{\Sigma}} x_i x_{i+j}}{n-k-j} = \frac{\overset{n-k-j}{\underset{i=1}{\Sigma}} x_{i+1} x_{i+j+1}}{n-k-j} = \ldots = \frac{\overset{n-k-j}{\underset{i=1}{\Sigma}} x_{i+k} x_{i+j+k}}{n-k-j} = \frac{\overset{n-j}{\underset{i=1}{\Sigma}} x_i x_{i+j}}{n-j}$$

für $j = 0, 1, 2, 3, \ldots$ algebraisch genau und daher wird (19) das Fehlerquadrat geringer erscheinen lassen, als es in Wirklichkeit ist. Verschiedene Überlegungen zeigen aber, daß die Einführung dieser Bedingungen bei kleinem k im Vergleich zu n das Fehlerquadrat des Gesamtausdruckes nur wenig berührt, und daher wollen wir annehmen, daß für eine "*oszillatorische*" Reihe, für welche $\Delta_2 = 0$, Formel (19) *in erster Annäherung* zum gewünschten Resultat führt.

Im Falle, wenn die Reihe $x_1', x_2', x_3', \ldots x_n'$ nicht nur "oszillatorisch" ist, sondern auch ein "normales" Verteilungsgesetz besitzt, ergiebt sich für (19) der folgende Ausdruck:

$$2\sigma_x^4 \left\{\frac{C_{4k}^{2k}}{(C_{2k}^k)^2} \cdot \frac{1}{(2k+1)^2}\left(\frac{k+1}{n-k-1} \cdot \frac{4k+1}{4k+2} - \frac{k}{n-k}\right) + \frac{(n-k)(n-k-1)-n}{2(n-k)^2(n-k-1)^2}\right\} \quad (20),$$

wobei: $C_{4k}^{2k} = \frac{(4k)!}{(2k)!\,(2k)!}$, und $C_{2k}^k = \frac{(2k)!}{k!\,k!}$.

Die Formel (20) ist genau und ohne Zuhilfenahme irgendwelcher Annäherungsformeln zustande gebracht. Beachtet man nun, daß bei Annahme $m = 0$ in (17 *a*)

$${}_0D_{k+1} = {}_kD_1 = -\frac{(k+1)\,\Delta^{2k} r'_{k+1}}{\Delta^{2k} r_k'} - k = -\frac{(k+1)\,\Delta^{2k} r'_{k+1} + k\Delta^{2k} r_k'}{\Delta^{2k} r_k'}$$

* Der Umstand, daß $\frac{E\left\{\overset{n-j}{\underset{i=1}{\Sigma}} x_i' x'_{i+j} - (n-j)\,x_{(n)}'^2\right\}}{n-j} \Big/ \frac{E\left\{\overset{n}{\underset{1}{\Sigma}} x_i'^2 - n x_{(n)}'^2\right\}}{n-1}$ (welcher Ausdruck in erster Annäherung gleich $E\rho_j'$ ist) für eine "oszillatorische" oder "uniforme" Reihe gleich $-\frac{1}{n}$ ist, kann hier nicht störend einwirken, da bei jeden konstanten ρ_0' alle ${}_0D_i$ in (18 *a*) ausgenommen ${}_0D_1$, gleich 0 werden.

sich von:

$$E\left\{\frac{\sum_{i=1}^{n-k-1}\Delta^{k+1}x_i^2}{\frac{(2k+2)!}{(k+1)!\,(k+1)!}(n-k-1)}-\frac{\sum_{i=1}^{n-k}\Delta^k x_i^2}{\frac{2k!}{k!\,k!}(n-k)}\right\}$$

$$=\frac{\sigma_x^2(-1)^k\Delta^{2k}r_k'}{\frac{(2k+1)!}{(k+1)!\,k!}}\left[-\frac{(k+1)\Delta^{2k}r'_{k+1}+k\Delta^{2k}r_k'}{(k+1)\Delta^{2k}r_k'}\right]\text{ (s. Form. 16 } a)$$

nur durch den Faktor

$$\frac{\sigma_x^2(-1)^k\Delta^{2k}r_k'}{\frac{(2k+1)!}{(k+1)!\,k!}(k+1)}=\frac{E\sum_{i=1}^{n-k}\Delta^k x_i^2}{\frac{2k!}{k!\,k!}(n-k)(2k+1)},$$

unterscheidet und daß letzterer für eine "*oszillatorische*" Reihe gleich $\sigma_x^2/2k+1$ wird, so ist es ersichtlich, daß aus (20) folgender Ausdruck für $\sigma^2_{{}_0D_{k+1}}$, die (*stand. deviation*)^2 von ${}_0D_{k+1}={}_kD_1$, abgeleitet werden kann:

$$\sigma^2_{{}_0D_{k+1}}=\frac{2C_{4k}^{2k}}{C_{2k}^kC_{2k}^k}\left(\frac{k+1}{n-k-1}\cdot\frac{4k+1}{4k+2}-\frac{k}{n-k}\right)+\frac{[(n-k)(n-k-1)-n](2k+1)^2}{(n-k)^2(n-k-1)^2} \quad \ldots\ldots\ldots\ldots\ldots(21).$$

Setzt man hier $k=0, 1, 2, 3, \ldots$, so erhält man die Ausdrücke für die Fehlerquadrate von ${}_0D_1$, ${}_0D_2$, ${}_0D_3$, ${}_0D_4$, ..., bei einer "oszillatorischen" und zugleich "normalen" Reihe. Nämlich:

$$\left.\begin{aligned}
\sigma^2_{{}_0D_1}&=\frac{n+1}{n(n-1)}-\frac{n}{n^2(n-1)^2};\\
\sigma^2_{{}_0D_2}&=\frac{2n+10}{(n-1)(n-2)}-\frac{9n}{(n-1)^2(n-2)^2};\\
\sigma^2_{{}_0D_3}&=\frac{49n+492}{18(n-2)(n-3)}-\frac{25n}{(n-2)^2(n-3)^2};\\
\sigma^2_{{}_0D_4}&=\frac{165n+2648}{50(n-3)(n-4)}-\frac{49n}{(n-3)^2(n-4)^2};\\
\sigma^2_{{}_0D_5}&=\frac{1859n+42.550}{490(n-4)(n-5)}-\frac{81n}{(n-4)^2(n-5)^2};\\
\sigma^2_{{}_0D_6}&=\frac{33.592n+1.023.483}{7938(n-5)(n-6)}-\frac{121n}{(n-5)^2(n-6)^2}
\end{aligned}\right\}\ldots\ldots\ldots(22).$$

Die zweiten Glieder, welche von der Ordnung $\frac{1}{n^3}$ sind, können in den meisten Fällen ohne Weiteres unterdrückt werden. Dann ergiebt sich angenähert:

$$\left.\begin{aligned}
\sigma_{{}_0D_1}&=\sqrt{\frac{n+1}{n(n-1)}}; & \sigma_{{}_0D_2}&=\sqrt{\frac{2n+10}{(n-1)(n-2)}};\\
\sigma_{{}_0D_3}&=\sqrt{\frac{2{,}72n+27{,}33}{(n-2)(n-3)}}; & \sigma_{{}_0D_4}&=\sqrt{\frac{3{,}30n+52{,}96}{(n-3)(n-4)}};\\
\sigma_{{}_0D_5}&=\sqrt{\frac{3{,}79n+86{,}84}{(n-4)(n-5)}}; & \sigma_{{}_0D_6}&=\sqrt{\frac{4{,}23n+128{,}93}{(n-5)(n-6)}},\ \text{u.s.w.}
\end{aligned}\right\}\ldots\ldots(23).$$

Dabei ist

$$\sigma_0 p_2 = \sigma_1 p_1 ; \quad \sigma_0 p_3 = \sigma_1 p_2 = \sigma_2 p_1 ; \quad \sigma_0 p_4 = \sigma_1 p_3 = \sigma_2 p_2 = \sigma_3 p_1 , \text{ u.s.w.}^*$$

Um die praktische Anwendung obiger Formeln zu illustrieren, sei folgendes kleines Beispiel angeführt.

Es wurden 50 mal je 4 Münzen zu gleicher Zeit geworfen und die Zahl der ausgefallenen Wappenseiten notiert. Raummangel ließ es aber nicht zu, die Münzen genügend hoch zu werfen, so daß eine gewisse positive Korrelation zwischen den Ergebnissen der einander nachfolgenden Würfe schon a priori zu erwarten war. Es ergab sich folgende Zahlenreihe: 2, 3, 1, 3, 4, 2, 0, 0, 3, 2, 1, 2, 4, 3, 2, 3, 2, 2, 2, 2, 3, 2, 2, 2, 4, 3, 1, 2, 2, 1, 2, 2, 3, 1, 2, 2, 1, 0, 1, 2, 1, 2, 3, 2, 1, 1, 1, 1, 4, 2 (Siehe Diagramm, Fig. 2.). Die mathematische Erwartung eines Wurfes wäre:

Fig. 2.

$\frac{1}{16} \cdot 0 + \frac{4}{16} \cdot 1 + \frac{6}{16} \cdot 2 + \frac{4}{16} \cdot 3 + \frac{1}{16} \cdot 4 = 2$; die theoretisch zu erwartende (stand. dev.)^2 der Reihe ist gleich $\sigma_x^2 = (\frac{1}{16} \cdot 0 + \frac{4}{16} \cdot 1^2 + \frac{6}{16} \cdot 2^2 + \frac{4}{16} \cdot 3^2 + \frac{1}{16} \cdot 4^2) - 2^2 = 1$.

* Die Formeln (11) und (13) erlauben auch ein Theorem zu beweisen, welches zur Verallgemeinerung der "Variate-Difference"-Methode beiträgt und als kleines Beispiel für die bei der Analyse der eingangserwähnten vier Reihen-Gruppen erforderlichen Verfahren dienen möge. Wenn *von einer gewissen k-ten Differenz angefangen* die x'-Reihe sich ständig "oszillatorisch" benimmt, so ist ${}_k r_j' = \frac{\Delta^{2k} r'_{k+j}}{\Delta^{2k} r_k} = (-1)^j \frac{C_{2k}^{k+j}}{C_{2k}^{k}}$.

Setzt man nun $\frac{\Delta^{2k} r_k'}{C_{2k}^{k}} = A$, so ist $\Delta^{2k} r'_{k+j} = (-1)^j A \cdot C_{2k}^{k+j}$. Es ist also $\Delta^{2k} r'_{2k+1} = 0$; $\Delta^{2k} r'_{2k} = (-1)^k A$; $\Delta^{2k} r'_{2k-1} = (-1)^{k-1} 2k A$; $\Delta^{2k} r'_{2k-2} = (-1)^{k-2} \cdot \frac{2k(2k-1)}{1 \cdot 2} A$, u.s.w. Bezeichnen wir die diesen r_i' entsprechenden Koeffizienten einer *von Anfang an* "oszillatorischen" Reihe durch R_i' und bilden wir die Reihe: $R'_{2k+1}, R'_{2k}, R'_{2k-1}, \ldots, R_2', R_1', R_0', R'_{-1}, R'_{-2}, R'_{-3}, \ldots$, so ist $R_0 = 1$ und alle übrigen R gleich Null. Bei der 1-ten Differenz sind alle $\Delta' R' = 0$, ausgenommen $\Delta' R_1' = -1$ und $\Delta' R_0' = +1$; für die $(2k)$-te Differenz erhalten wir folgende Werte: $\Delta^{2k} R'_{2k+1} = 0$; $\Delta^{2k} R'_{2k} = (-1)^k$; $\Delta^{2k} R'_{2k-1} = (-1)^{k-1} \cdot 2k$; $\Delta^{2k} R'_{2k-2} = (-1)^{k-2} \cdot \frac{2k(2k-1)}{1 \cdot 2}$, u.s.w. Folglich ist: $\Delta^{2k} r'_{2k+1} = \Delta^{2k} R'_{2k+1} = 0$; $\Delta^{2k} r'_{2k} = A \Delta^{2k} R'_{2k}$, $\Delta^{2k} r'_{2k-1} = A \Delta^{2k} R'_{2k-1}, \ldots$, $\Delta^{2k} r_j' = A \Delta^{2k} R_j'$. Daraus ist ersichtlich, daß jedes r_i' in der r'-Reihe entweder dem entsprechenden R_i' in der R'-Reihe proportional und somit, ausgenommen r_0', gleich 0 ist; oder aber besitzt jedes r_i' eine Komponente, die eine Funktion der Ordnungszahl i ist und bei der $(2k)$-ten Differenz gänzlich verschwindet. Mit anderen Worten, ist dann die Reihe $r'_{2k+1}, r'_{2k}, r'_{2k-1}, r'_{2k-2}, \ldots$ durch eine *Parabel jedenfalls nicht höher als* $(2k-1)$*-ter Ordnung* darstellbar. Die Untersuchung der möglichen Formen dieser Parabel sowie die Anwendung des Theorems auf die r-Reihe würde uns zu weit führen.

Aus der Reihe selbst ergaben sich folgende Größen:

$$x_{(n)} = \frac{\sum_{1}^{50} x_i}{50} = 1{,}98; \quad \frac{\sum_{i=1}^{50} (x_i - x_{(n)})^2}{49} = 0{,}9998; \quad \rho_1' = +0{,}18; \quad \rho_2' = -0{,}36;$$

$$\rho_3' = -0{,}05; \quad \rho_4' = +0{,}10; \quad \rho_5' = -0{,}25; \quad \rho_6' = -0{,}35.$$

Setzt man diese Werte in die Formeln (18a) statt der entsprechenden r_j' und $n = 50$ in (23), so erhält man: ${}_0D_1 = -0{,}18$ und $\sigma_{{}_0D_1} = 0{,}14$; ${}_0D_2 = -0{,}66$ und $\sigma_{{}_0D_2} = 0{,}21$; ${}_0D_3 = -0{,}95$ und $\sigma_{{}_0D_3} = 0{,}27$; ${}_0D_4 = -1{,}09$ und $\sigma_{{}_0D_4} = 0{,}32$; ${}_0D_5 = -1{,}08$ und $\sigma_{{}_0D_5} = 0{,}36$; ${}_0D_6 = -0{,}99$ und $\sigma_{{}_0D_6} = 0{,}42$. Das Fehlerquadrat der Reihe wächst also bis zur 6-ten endlichen Differenz inklusive langsamer, als es einer "oszillatorischen" Reihe zukommt. Die Grenze der negativen D ist, wie wir wissen, gleich $-(2k + 2m + 1)$, was -7 für ${}_0D_4$, -9 für ${}_0D_5$ und -11 für ${}_0D_6$ ausmacht. Es ist aber $\frac{{}_0D_4}{7} = -0{,}16$; $\frac{{}_0D_5}{9} = -0{,}12$ und $\frac{{}_0D_6}{11} = -0{,}09$, also kleiner, als $\frac{{}_0D_1}{1} = -0{,}18$.

Berechnen wir nun für unsere Reihe die Größen:

$$\frac{\sum_{i=1}^{n} (x_i - x_{(n)})^2}{n-1}, \quad \frac{\sum_{i=1}^{n-1} \Delta' x_i^2}{2(n-1)}, \cdots \frac{\sum_{i=1}^{n-6} \Delta^{\text{VI}} x_i^2}{924(n-6)},$$

so erhalten wir wirklich: 1,000; 0,816; 0,677; 0,540; 0,423; 0,342; 0,300.

Da das Fehlerquadrat $\sigma^2_{\sigma_k^2}$ für

$$\frac{\sum_{i=1}^{n-k} \Delta^k x_i^2}{\frac{2k!}{k!\,k!}(n-k)}$$

bei einer *normalen* Verteilung der x durch die Formel:

$$\sigma^2_{\sigma_k^2} = \frac{2\sigma_x^4}{n-k}\left[\frac{C_{4k}^{2k}}{(C_{2k}^k)^2} - \frac{k}{2(n-k)}\right]$$

gegeben ist, so würde also $\sigma_{\sigma_k^2}$ für die 6-te Differenz bei $n = 50$ etwa 0,375 ausmachen, was auf $\sigma_x^2 = 1$ bezogen werden müßte. Somit gehört unsere Reihe wohl schwerlich der "R"-Gruppe an.

Nun seien noch die Formeln (18b) angewandt. Für die 3-tten Differenzen unserer Reihe ergeben sich folgende Werte: ${}_3\rho_1' = -0{,}47$; ${}_3\rho_2' = -0{,}18$; ${}_3\rho_3' = +0{,}09$. Setzt man diese Größen und $k = 3$ in (18b) ein, so erhält man: ${}_3D_1 = -1{,}12$ und $\frac{{}_3D_1}{7} = -0{,}16$; ${}_3D_2 = -1{,}01$ und $\frac{{}_3D_2}{9} = -0{,}11$; ${}_3D_3 = -0{,}85$ und $\frac{{}_3D_3}{11} = -0{,}08$. Die Größen ${}_3D_1$ und ${}_0D_4$, ${}_3D_2$ und ${}_0D_5$, ${}_3D_3$ und ${}_0D_6$, die einander ungefähr gleich sein müssen, weichen im Vergleich zu deren Fehlergrenzen nur unbeträchtlich von einander ab.

Laut Form. (11) ist

$$ {}_3r_1' = \frac{\Delta^{\text{vi}} r_4'}{\Delta^{\text{vi}} r_3'}, \quad {}_3r_2' = \frac{\Delta^{\text{vi}} r_5'}{\Delta^{\text{vi}} r_3'}, \quad {}_3r_3' = \frac{\Delta^{\text{vi}} r_6'}{\Delta^{\text{vi}} r_3'}. $$

Setzt man hier statt r_i' die entsprechenden ρ_i' ein, so erhält man:

$$ {}_3\rho_1' = \frac{0{,}10 + 6 \,.\, 0{,}05 - 15 \,.\, 0{,}36 - 20 \,.\, 0{,}18 + 15 - 6 \,.\, 0{,}18 - 0{,}36}{-2 \,.\, 0{,}05 + 12 \,.\, 0{,}36 + 30 \,.\, 0{,}18 - 20} = -0{,}48, $$

während wir direkt aus den 3-tten Differenzen ${}_3\rho_1' = -0{,}47$ erhielten. Ebenso erhält man ${}_3\rho_2' = -0{,}22$ und ${}_3\rho_3' = +0{,}16$, während wir direkt aus den 3-tten Differenzen ${}_3\rho_2' = -0{,}18$ und ${}_3\rho_3' = +0{,}09$ hatten. Die Übereinstimmung ist für ${}_3\rho_1'$ und ${}_3\rho_2'$ ziemlich befriedigend; ${}_3\rho_3'$ ist aber zu klein, um bei seiner Bestimmung eine größere Genauigkeit erwarten zu können.

Geben wir jetzt derselben Reihe von 50 Versuchen eine "Z"-Komponente $+1, -1, +1, -1, +1, -1, \ldots$ (d. h. eine Sinus-Reihe mit einem Intervall von einer halben Periode) zu. Wir erhalten die Reihe: 3, 2, 2, 2, 5, 1, 1, −1, 4, 1, 2, 1, 5, 2, 3, 2, 3, 1, 3, 1, 4, 1, 3, 1, 5, 2, 2, 1, 3, 0, 3, 1, 4, 0, 3, 1, 2, −1, 2, 1, 2, 1, 4, 1, 2, 0, 2, 0, 5, 1 (Diagramm, siehe Fig. 3). Für die 1-te Differenz ergiebt sich:

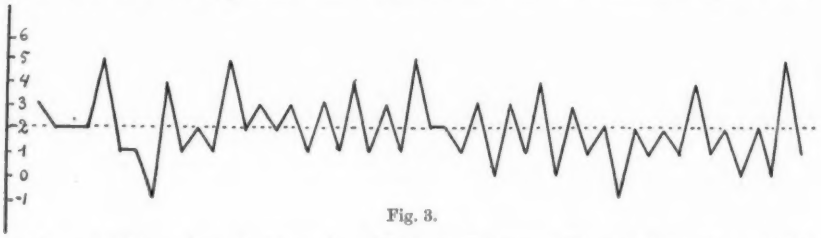


Fig. 3.

${}_1\rho_1' = -0{,}808$; ${}_1\rho_2' = +0{,}632$; ${}_1\rho_3' = -0{,}736$; und daraus: ${}_1D_1 = +0{,}615$; ${}_1D_2 = +0{,}695$; ${}_1D_3 = +0{,}765$. Beim Übergange zur 2-ten Differenz ist also die Anwesenheit einer starken "Z"-Komponente wahrnehmbar, welche bei jeder der nachfolgenden zwei Differenzen nur bemerkbarer wird. Es hat keinen Sinn die Kriterien für die 5-te, 6-te und ff. Differenzen zu bestimmen. Unmittelbare Rechnung ergiebt:

$$ \frac{\sum_{i=1}^{n} (x_i' - x_{(n)})^2}{n-1} = 2{,}22; \quad \frac{\sum_{i=1}^{n-1} \Delta' x_i^2}{2(n-1)} = 3{,}22; \quad \frac{\sum_{i=1}^{n-2} \Delta'' x_i^2}{6(n-2)} = 3{,}87; \quad \frac{\sum_{i=1}^{n-3} \Delta''' x_i^2}{20(n-3)} = 4{,}35. $$

Das Kriterium D bewährt sich hier vollauf. Beachten wir noch, daß das Kriterium D mit jeder Differenz zunimmt, während absolut und prozentuell die Differenzen zwischen 2,22 und 3,22; 3,22 und 3,87, u.s.w., geringer werden.

Ein anderes Beispiel. In seiner Tab. XI hat Yule* für die Korrelation zwischen der m-ten und $(m+n)$-ten *sechsten* Differenz der Daten für Kindersterblichkeit in England folgende Zahlen angegeben:

$$ n = 1, \quad 2, \quad 3, \quad 4, $$
$$ \rho_n' = -0{,}906; \; +0{,}678; \; -0{,}469; \; +0{,}288, \text{ u.s.w.} $$

Vorausgesetzt, daß bei der Berechnung dieser Größen kein Fehler unterlaufen ist, würden sie, in (18b) eingefügt, folgende Resultate ergeben: ${}_6D_1 = +0{,}342$;

* G. U. Yule, "On the Time-correlation Problem, etc.," *Journ. Roy. Stat. Soc.* Vol. LXXXIV, 497 ff.

${}_6D_2 = +0{,}324$; ${}_6D_3 = +0{,}334$; ${}_6D_4 = +0{,}334$. Auch in der (6+4=) 10-ten Differenz wäre da die Anwesenheit eines "Z"-Elements unverkennbar.

Abgesehen von ihrer Hauptaufgabe, die schwierige Berechnung der höheren endlichen Differenzen zu ersetzen, können die Kriterien (18 a) und (18 b) auch die Analyse der "episäkularen" Reihen vom Typus "S" erleichtern. Ohne auf diesen Gegenstand einzugehen, erlauben wir uns hier nur eine kurze Andeutung. Ist 00 die Null-Linie (siehe Fig. 4), so könnten 1—1, 2—2, 3—3 einige denkbare Typen der D-Reihen schematisch darstellen. 1—1 entspräche etwa der Kombination "G"+"R," 2—2 der Kombination "G"+"Z," 3—3 derjenigen von "G"+"R"+"Z," u.s.w.

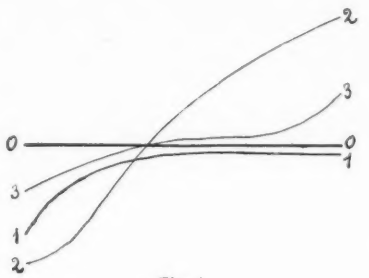


Fig. 4.

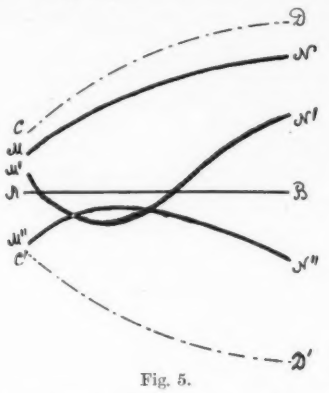


Fig. 5.

Es muß aber eins berücksichtigt werden: das Bild kann durch die sehr große positive Korrelation zwischen den Größen

$$\frac{\sum_{i=1}^{n-1} \Delta' x_i^2}{2(n-1)}, \quad \frac{\sum_{i=1}^{n-2} \Delta'' x_i^2}{6(n-2)}, \quad \frac{\sum_{i=1}^{n-3} \Delta''' x_i^2}{20(n-3)}, \cdots$$

(aus deren Differenzen unsere D-Kriterien entstehen) verdunkelt werden.

Der Korrelationskoeffizient, welcher den Zusammenhang zwischen den Größen

$$\frac{\sum_{i=1}^{n-k} \Delta^k x_i^2}{\frac{2k!}{k!\,k!}(n-k)} \quad \text{und} \quad \frac{\sum_{i=1}^{n-k-1} \Delta^{k+1} x_i^2}{\frac{(2k+2)!}{(k+1)!\,(k+1)!}(n-k-1)}$$

charakterisiert, kann für eine "oszillatorische" und "normale" x-Reihe mit einiger Annäherung gleich $\sqrt{1 - \frac{1}{(2k+1)(4k+3)}}$ gesetzt werden, und wenn die Linie AB (siehe Fig. 5) die Größe σ_x^2 für eine "oszillatorische" Reihe darstellt und CD und $C'D'$ die Fehlergrenzen, die bei der Bestimmung dieser Grösse aus der 1-ten, 2-ten, 3-tten, ... k-ten endl. Differenz zulässig sind, so würden die Linien MN, $M'N'$, $M''N''$, ... einige mögliche Typen solcher Differenzen-Fehlerquadrat-Kurven andeuten.

BUDAPEST, *Juni* 1923

MISCELLANEA.

I. On Hierarchical Order among Correlation Coefficients.

By GODFREY H. THOMSON, Armstrong College, Newcastle-upon-Tyne.

In *Biometrika*, Vol. XII, page 355, in an article which criticised certain formulae used by Professor Spearman to measure "hierarchical order" among correlation coefficients, there occurred some arithmetical errors which Professor Spearman has pointed out, in the second example given on pages 361—363. The object of the present paper is to correct those slips, which I much regret, and also others which have been discovered. It will be seen that their effect was not great, and that the present corrected and extended calculations, which have been subjected to very careful checks (not only by myself but by others), support equally the conclusion drawn in the original paper.

Professor Spearman's thesis is that the "intercolumnar correlation" among the correlation coefficients found in psychological tests is really very high indeed, and from this he deduces the existence of "general ability" and the non-existence of any other factors except specific factors. By the intercolumnar correlation is meant the correlation between the columns, taken in pairs in all possible ways, of a square table such as this:

	x_1	x_2	x_3	•	•	x_k
x_1	—	$r_{1\cdot2}$	$r_{1\cdot3}$	•	•	$r_{1\cdot k}$
x_2	$r_{1\cdot2}$	—	$r_{2\cdot3}$	•	•	$r_{2\cdot k}$
x_3	$r_{1\cdot3}$	$r_{2\cdot3}$	—	•	•	$r_{3\cdot k}$
•	•	•	•	—	•	•
•	•	•	•	•	—	•
x_k	$r_{1\cdot k}$	$r_{2\cdot k}$	$r_{3\cdot k}$	•	•	—

where $x_1, x_2, \ldots x_k$ are the scores in the psychological tests, and the r's their correlations with one another.

Professor Spearman gives a formula (quoted below, p. 152), for "correcting" the *observed* intercolumnar correlations: and my example was directed to testing this formula by applying it to a case where the *true*, as well as the observed, values were known, namely ten variates composed of dice throws. The composition of the variates was given in an earlier paper (*Proc. Roy. Soc.* A., XCV, 1919, 403) in a table* in which the following corrections should be made:

Correlation between	For	Read
x_3 and x_9	0·62	0·47
x_2 and x_3	0·39	0·36
x_5 and x_{10}	0·41	0·42
x_8 and x_{10}	0·15	0·14

* This table and others referred to can also be found on pages 175—179 of *The Essentials of Mental Measurement*, Brown and Thomson, Cambridge, 1921. The method adopted can be perhaps best understood, by a reader unacquainted with the papers referred to, by turning direct to the new example worked in the present paper after Table E.

Dice were then actually thrown to obtain observed values of the correlations. The dice throws are given in *Biometrika, loc. cit.*, and on page 362 the following corrections are needed:

In Table IV.

Subject	Test	For	Read
1	x_3	37	43
1	x_7	42	48
2	x_3	42	44
2	x_7	46	48
10	x_9	28	26

The last is a misprint and is not carried on to the succeeding tables: but as regards subjects 1 and 2 corresponding alterations are needed in Table V.

The above corrections would not have exerted any large influence on the final results. The mistake which Professor Spearman has detected is in the decision as to which pairs of columns in Table VI pass his correctional standard. Here, in order to use certain tables, I omitted temporarily a factor 0·67449, and in allowing for this I used this factor plain instead of squared, as I ought. This error was corrected later while rechecking, and the final values of the columnar correlations are free from it: but the decision as to which of the columns pass the "correctional standard" had meanwhile been made. Really, although five pairs of columns almost pass that standard, none of them quite does so, and the calculations up to this point are consequently inconclusive.

I have therefore extended them, by throwing more dice, so as to cover 30 subjects instead of 20. The following tables show the data thus obtained.

TABLE III. *Biometrika*, Vol. XII, p. 361 (*continued*), showing new dice throws for Group Factors.

Number identifying the subject	Name of Group Factor *												
	Ace	2	3	4	5	6	7	8	9	10	Kn	Q	K
21	4	4	2	3	4	5	3	2	1	6	6	3	4
22	1	3	3	2	6	5	5	2	4	5	3	1	5
23	5	6	4	6	3	2	3	1	4	4	6	6	1
24	6	4	1	2	4	4	6	4	4	3	1	3	1
25	1	1	3	3	2	1	2	3	5	6	2	1	1
26	6	4	4	5	6	1	5	5	1	2	1	3	6
27	1	6	4	2	3	6	4	6	5	5	3	1	2
28	4	2	4	6	1	1	4	3	4	4	3	6	6
29	3	4	5	5	6	6	3	6	3	4	5	5	4
30	3	5	5	3	5	1	3	1	3	2	3	4	4

* The Group Factors are given the names of playing cards because cards were drawn to identify those Group Factors occurring in each variate. The numbers in the body of the table are dice throws.

Table V is obtained from Table IV by adding, to each Group Factor score, the score of a number of dice representing the specific factors, as described in the articles quoted, or in *The Essentials of Mental Measurement*, page 175.

With these scores, together with those given in the previous articles for the first 20 subjects, are obtained "observed" values of the correlations, whose true values are already known. The

product-moment formula was used, in which each correlation is obtained in the form

$$r = \frac{S(hk)}{\sqrt{\{S(h^2) \cdot S(k^2)\}}},$$

where h and k represent any two of the variates x, measured from their respective means. The correlations obtained were as follows (Table A):

TABLE IV. *Biometrika*, Vol. XII, p. 362 (*continued*).

Number identifying the subject	Scores in the Group Factor portion of the test									
	x_1	x_2	x_3	x_4	x_5	x_6	x_7	x_8	x_9	x_{10}
21	19	17	42	3	24	33	47	4	31	42
22	20	21	40	1	23	33	45	3	32	40
23	18	20	49	6	25	35	51	6	31	42
24	21	20	39	3	28	31	43	4	32	33
25	7	16	30	1	11	25	31	1	17	25
26	27	17	48	3	35	29	49	4	42	42
27	16	18	42	1	23	35	48	6	34	42
28	17	19	47	6	26	32	48	2	31	40
29	20	21	53	5	31	43	59	4	42	53
30	20	17	41	4	25	25	42	5	30	36

TABLE V. *Biometrika*, Vol. XII, p. 362 (*continued*).

Number identifying the subject	Total Scores in the tests									
	x_1	x_2	x_3	x_4	x_5	x_6	x_7	x_8	x_9	x_{10}
21	33	25	85	15	42	54	82	12	42	62
22	47	32	87	11	47	53	84	5	41	59
23	32	29	77	15	48	57	98	14	42	63
24	41	31	87	16	50	46	99	13	43	58
25	23	30	70	8	26	39	77	10	23	45
26	44	26	99	9	56	47	95	14	54	63
27	39	28	92	14	51	51	91	14	41	59
28	41	32	102	16	41	52	91	7	43	58
29	40	29	94	14	57	57	107	7	55	68
30	33	28	84	12	51	44	86	17	37	50

The intercolumnar correlations of this square table can now be found, for the observed and the true values separately, ignoring in each column that correlation which has no partner in the column with which it is being paired. The product-moment formula was again used. Tables B and C show the values of these intercolumnar correlations R.

For Spearman's "corrected" values R' there are needed certain corrections. His formula is:

$$R'_{ab} = \frac{S(\rho_{xa}\rho_{xb}) - (n-1)\, r_{ab}\sigma_{xa}\sigma_{xb}}{\sqrt{\{S(\rho^2_{xa}) - (n-1)\,\overline{\sigma^2_{xa}}\}}\,\sqrt{\{S(\rho^2_{xb}) - (n-1)\,\overline{\sigma^2_{xb}}\}}},$$

where the σ's are the standard deviations of the r's, the bar indicates mean values, n is the number (eight) of pairs of correlation coefficients in the two columns, and a and b are any two of the columns. The formula is the usual product-moment formula with certain corrections subtracted from the numerator and from each of the denominator factors.

TABLE A*.

Observed values (above) and theoretical values (below) of the correlations of the ten variates in pairs.

	x_1	x_2	x_3	x_4	x_5	x_6	x_7	x_8	x_9	x_{10}
x_1	—	23	59	27	45	28	33	−06	58	45
	—	22	32	00	44	17	31	18	46	32
x_2	23	—	23	04	33	58	43	−14	28	37
	22	—	36	18	29	47	35	00	20	35
x_3	59	23	—	−14	36	42	58	05	40	52
	32	36	—	10	39	44	48	12	47	51
x_4	27	04	−14	—	25	−01	−11	−10	10	24
	00	18	10	—	14	13	10	00	00	12
x_5	45	33	36	25	—	57	52	19	67	66
	44	29	39	14	—	30	38	16	40	42
x_6	28	58	42	−01	57	—	57	01	49	71
	17	47	44	13	30	—	47	00	38	53
x_7	33	43	58	−11	52	57	—	02	58	69
	31	35	48	10	38	47	—	11	51	54
x_8	−06	−14	05	−10	19	01	02	—	03	−07
	18	00	12	00	16	00	11	—	17	14
x_9	58	28	40	10	67	49	58	03	—	61
	46	20	47	00	40	38	51	17	—	58
x_{10}	45	37	52	24	66	71	69	−07	61	—
	32	35	51	12	42	53	54	14	58	—

* This and the following tables are distinguished by Roman capitals to avoid confusion with the tables of the original paper, some of which have been continued in the present paper in previous paragraphs.

TABLE B.

Observed Intercolumnar Correlations.

Average value = ·63.

	1	2	3	4	5	6	7	8	9	10
1	•	·51	·56	·33	·64	·59	·70	·54	·70	·73
2	·51	•	·73	·18	·81	·92	·81	·31	·81	·95
3	·56	·73	•	·41	·78	·72	·83	·29	·89	·73
4	·33	·18	·41	•	·49	·33	·29	·00	·59	·23
5	·64	·81	·78	·49	•	·76	·81	·45	·91	·87
6	·59	·92	·72	·33	·76	•	·94	·27	·78	·83
7	·70	·81	·83	·29	·81	·94	•	·40	·81	·84
8	·54	·31	·29	·00	·45	·27	·40	•	·59	·73
9	·70	·81	·89	·59	·91	·78	·81	·59	•	·89
10	·73	·95	·73	·23	·87	·83	·84	·73	·89	•

TABLE C.

True Intercolumnar Correlations.

Average value = ·59.

	1	2	3	4	5	6	7	8	9	10
1	•	·20	·71	−·17	·86	·52	·70	·89	·79	·71
2	·20	•	·75	·80	·53	·89	·75	−·31	·63	·73
3	·71	·75	•	·42	·84	·88	·99	·43	·87	·98
4	−·17	·80	·42	•	−·01	·67	·37	−·69	·06	·27
5	·86	·53	·84	−·01	•	·65	·82	·86	·93	·77
6	·52	·89	·88	·67	·65	•	·88	·05	·62	·83
7	·70	·75	·99	·37	·82	·88	•	·42	·86	·99
8	·89	−·31	·43	−·69	·86	·05	·42	•	·74	·37
9	·79	·63	·87	·06	·93	·62	·86	·74	•	·84
10	·71	·73	·98	·27	·77	·83	·99	·37	·84	•

TABLE D.

Spearman "corrected" Intercolumnar Correlations.

Average value = ∞.

Median value = 1·24.

i = imaginary, one factor under the square root being negative.

nn = both factors under the square root negative. Arithmetically possible but meaningless.

	1	2	3	4	5	6	7	8	9	10
1	•	·83	·58	*i*	·91	·83	·96	*nn*	·92	·98
2	·83	•	1·13	*i*	1·47	1·76	1·15	*nn*	1·24	1·39
3	·58	1·13	•	*i*	1·20	·87	·98	*i*	1·14	·85
4	*i*	*i*	*i*	•	*i*	*i*	*i*	*nn*	*i*	*i*
5	·91	1·47	1·20	*i*	•	1·06	1·15	*i*	1·68	1·52
6	·83	1·76	·87	*i*	1·06	•	1·08	*i*	·92	·90
7	·96	1·15	·98	*i*	1·15	1·08	•	*i*	·93	·91
8	*nn*	*nn*	*i*	*nn*	*i*	*i*	*i*	•	*i*	*i*
9	·92	1·24	1·14	*i*	1·68	·92	·93	*i*	•	1·04
10	·98	1·39	·85	*i*	1·52	·90	·91	*i*	1·04	•

All the Spearman corrected values are given here: but Professor Spearman only admits those in which the correction of each factor of the denominator is less than half the factor to be corrected. Ten pairs of columns pass this requirement, and are given in Table E, p. 155.

Professor Spearman's formula would therefore have claimed these dice throws as giving practically perfect "hierarchical order" among their correlation coefficients (intercolumnar correlation = unity): whereas the true intercolumnar correlation is known to be of an average value of 0·59.

Since Professor Spearman has also criticised the fact that only one example has been brought forward by me (really two, for I gave another) I have worked out a new example given below. It was necessary to go up to 60 subjects before any pairs of columns passed his standard and then only three pairs did so. The length of time required however to throw the dice and make the calculations prevents me from providing further examples, nor do I wish to spend further time over a formula which is, I am convinced, utterly misleading.

TABLE E.

Columns passing the standard	Observed R	True R	Spearman's R'
3 and 6	·72	·88	·87
3 „ 7	·83	·99	·98
3 „ 9	·89	·87	1·14
3 „ 10	·73	·98	·85
6 „ 7	·94	·88	1·08
6 „ 9	·78	·62	·92
6 „ 10	·83	·83	·90
7 „ 9	·81	·86	·93
7 „ 10	·84	·99	·91
9 „ 10	·89	·84	1·04
Average	·83	·87	·96

Observed intercolumnar correlation R over whole table ... ·63
True R over whole table ... ... ·59
Observed R in columns selected by Spearman's standard ... ·83
True R in those columns ... ... ·87
Spearman's "corrected" intercolumnar correlation R' ... ·96

New example.

Each variate is to contain some dice throws which are specific to that variate alone, and some which are to be common to a group of variates, though not general to all. The number of each kind of dice was determined by shuffling and cutting a pack of cards, counting Knave, Queen, King as 11, 12, 13, with the following result:

TABLE F.

Variate	Number of Group Factors	Number of Specific Factors
x_1	7	4
x_2	5	13
x_3	10	9
x_4	9	12
x_5	6	9
x_6	11	10
x_7	12	8
x_8	13	9
x_9	6	12
x_{10}	5	1

To identify the group factors of each variate the group factors were given the names of the 13 cards of a suit. Such a pack of 13 cards was then shuffled, and 7 cards dealt out to represent the group factors of variate x_1, with the result shown in the top line of the next table. In a similar way, by shuffling and dealing, the group factors of each variate were identified:

TABLE G.

Variate	Name of Group Factor												
	Ace	2	3	4	5	6	7	8	9	10	Kn	Q	K
x_1	×	×		×			×	×	×	×			
x_2		×	×			×	×			×			
x_3		×	×	×	×	×	×	×	×	×			×
x_4	×		×	×	×	×		×		×	×	×	
x_5		×	×		×	×					×		×
x_6	×		×	×	×	×	×	×	×	×	×	×	
x_7	×	×	×	×	×	×	×	×	×	×	×		×
x_8	×	×	×	×	×	×	×	×	×	×	×	×	×
x_9		×		×				×		×	×		×
x_{10}		×				×	×			×			×

Thus the die represented by the name Ace is to be counted as a factor of the variates x_1, x_4, x_6, x_7, x_8, and so on.

The structure of these variates is to remain the same for each "subject," but the dice will be thrown afresh for each new subject. The dice throws representing the group factors for 60 subjects in a certain trial are given in Table H.

From these dice throws there can be found, according to Table G, that portion of the total "score" of each "subject" in each variate which is due to his group factors. For example in the case of subject 1, variate x_1, it is

$$5+3+4+1+4+6+4=27.$$

To this has to be added the score of 4 dice (in accordance with Table F) which are entirely specific to this variate, and which in the case of subject 1 gave

$$1+2+4+5=12.$$

The total score of subject 1 in variate x_1 is therefore $27+12=39$. In this way the following scores were obtained. It is not practicable to print all the remaining dice throws, 5220 in number, but a copy has been deposited with the Editor of this Journal. The total scores thus obtained in the above trial are given in Table I, p. 158.

From these scores the correlations of the variates with one another can be found; by the product-moment formula they are as follows:

TABLE J (formed from Table I).

Observed Correlations of the Variates.

	x_1	x_2	x_3	x_4	x_5	x_6	x_7	x_8	x_9	x_{10}
x_1	•	·32	·50	·33	·16	·23	·39	·44	·47	·54
x_2	·32	•	·61	·24	·15	·20	·48	·49	·14	·58
x_3	·50	·61	•	·36	·24	·18	·58	·49	·26	·59
x_4	·33	·24	·36	•	·15	·24	·48	·40	·22	·32
x_5	·16	·15	·24	·15	•	·14	·38	·28	·26	·32
x_6	·23	·20	·18	·24	·14	•	·44	·50	·14	·21
x_7	·39	·48	·58	·48	·38	·44	•	·64	·32	·60
x_8	·44	·49	·49	·40	·28	·50	·64	•	·31	·59
x_9	·47	·14	·26	·22	·26	·14	·32	·31	•	·31
x_{10}	·54	·58	·59	·32	·32	·21	·60	·59	·31	•

TABLE H.

Subject	Name of Group Factor												
	Ace	2	3	4	5	6	7	8	9	10	Kn	Q	K
1	5	3	1	4	5	1	1	4	6	4	4	1	5
2	2	3	3	3	6	6	4	6	5	5	1	4	4
3	5	2	5	1	2	4	5	3	1	1	1	5	5
4	4	3	6	5	1	1	5	2	5	3	5	4	5
5	4	5	6	6	6	5	4	2	6	6	1	4	6
6	4	3	5	4	1	4	3	5	3	2	6	4	1
7	5	6	4	4	2	5	1	4	5	2	4	2	6
8	1	2	5	3	5	1	2	6	3	1	3	6	1
9	2	4	3	2	1	1	4	3	4	4	5	2	6
10	1	1	4	4	5	1	6	5	3	2	4	4	3
11	6	6	6	4	5	6	6	5	1	4	2	5	4
12	4	6	2	2	5	4	1	4	3	4	3	1	1
13	4	2	5	2	1	4	4	2	4	2	6	3	3
14	1	4	4	5	1	6	5	5	3	6	2	3	3
15	5	4	5	3	1	5	5	4	6	1	1	4	1
16	1	1	2	3	4	4	4	4	5	6	3	3	4
17	6	5	5	3	5	2	1	1	6	6	5	1	4
18	5	5	5	2	3	5	3	6	1	5	6	5	6
19	1	5	6	3	6	4	4	4	4	6	6	6	1
20	5	3	4	1	4	1	3	2	1	3	1	1	4
21	2	6	1	5	2	1	2	5	6	3	3	6	3
22	4	6	4	6	2	4	1	3	1	5	2	3	4
23	4	3	1	4	2	5	5	6	5	6	5	3	1
24	5	6	6	5	6	3	3	6	4	6	3	5	5
25	2	1	2	5	6	6	2	4	5	4	3	3	6
26	6	4	3	4	3	2	3	3	4	5	3	5	5
27	4	2	4	4	4	1	1	2	6	5	5	5	6
28	3	3	1	3	3	1	4	5	5	4	4	5	6
29	2	6	3	5	5	1	6	5	1	5	2	2	1
30	4	1	4	5	4	4	6	3	2	3	3	2	6
31	1	3	2	6	3	2	1	4	1	3	3	4	4
32	3	3	3	2	4	4	5	2	3	2	5	4	5
33	4	4	3	3	2	2	3	5	3	2	1	2	2
34	4	1	4	4	1	5	6	3	1	4	1	5	1
35	5	2	3	4	6	2	4	3	1	5	1	4	3
36	6	5	3	3	2	6	5	4	2	5	3	5	2
37	5	3	4	2	6	2	4	5	4	2	5	3	6
38	1	1	3	3	6	2	5	5	6	3	4	5	4
39	2	3	4	6	3	4	5	5	5	1	5	3	1
40	2	5	4	5	2	6	1	3	6	3	1	4	5
41	1	5	6	5	5	4	5	1	3	5	4	5	4
42	1	4	2	3	6	3	1	3	3	3	6	5	5
43	4	2	4	1	2	3	1	1	1	1	4	3	4
44	4	1	6	1	6	4	4	6	2	2	6	6	4
45	3	5	5	3	4	1	6	2	5	3	6	2	5
46	2	3	4	5	3	3	4	4	6	1	6	1	5
47	5	3	6	5	6	6	3	1	3	1	2	4	3
48	1	3	3	6	5	6	1	3	3	4	1	3	4
49	3	4	4	2	3	3	2	4	2	3	1	1	1
50	4	6	6	5	5	3	2	3	1	2	1	6	5
51	2	3	1	2	6	4	3	2	5	4	4	2	4
52	1	1	3	6	4	4	2	2	5	6	1	5	1
53	4	1	1	4	3	3	6	5	4	3	5	2	4
54	6	1	4	4	1	4	5	6	2	6	1	6	1
55	5	3	6	5	5	6	3	1	5	2	1	2	6
56	5	3	4	5	3	5	4	3	3	5	4	2	2
57	2	1	5	3	6	4	4	2	6	5	4	1	4
58	3	4	4	6	1	1	3	5	5	1	6	6	6
59	1	5	4	5	5	3	3	6	4	3	3	4	4
60	4	3	1	3	4	4	5	3	4	2	3	5	5

TABLE I.

Subject	Variate									
	x_1	x_2	x_3	x_4	x_5	x_6	x_7	x_8	x_9	x_{10}
1	39	57	66	78	62	71	70	78	74	18
2	42	61	73	88	62	78	77	79	62	23
3	30	72	69	71	55	71	61	74	55	21
4	40	62	72	79	56	84	71	85	60	20
5	48	73	89	82	68	72	82	95	56	30
6	41	56	59	71	47	83	67	83	71	18
7	38	65	70	66	51	75	78	83	64	22
8	31	63	69	67	41	70	67	60	51	8
9	39	62	66	71	56	61	69	68	78	24
10	36	64	70	69	55	75	60	72	64	15
11	43	71	77	86	62	85	82	91	68	28
12	38	61	62	78	59	71	73	70	67	19
13	35	63	58	77	60	78	66	81	58	18
14	48	73	82	69	55	65	65	78	69	30
15	43	72	72	74	48	66	71	81	46	22
16	41	56	67	87	49	77	74	67	67	20
17	45	59	70	88	59	69	77	80	66	19
18	41	70	66	83	59	85	87	93	66	29
19	45	75	73	67	64	81	76	90	75	23
20	29	53	54	64	43	70	63	73	53	17
21	46	66	65	68	52	72	70	77	73	20
22	43	71	66	86	51	67	73	79	75	21
23	52	64	76	88	52	70	70	84	79	25
24	45	75	87	86	56	94	89	97	75	28
25	32	65	73	84	52	74	76	85	65	20
26	39	56	61	85	49	67	66	93	65	20
27	39	59	78	80	52	75	77	78	64	19
28	40	65	71	67	49	80	68	86	68	21
29	45	66	69	77	51	72	73	82	67	24
30	34	72	75	87	49	75	77	80	59	25
31	33	48	62	75	48	71	59	67	51	16
32	36	63	66	66	56	78	65	72	68	24
33	41	51	64	48	49	69	66	73	65	18
34	33	68	63	83	41	72	62	85	65	20
35	39	59	67	72	54	71	72	68	60	22
36	45	73	70	79	52	78	74	87	61	24
37	43	57	62	76	62	80	68	79	64	20
38	34	49	65	68	60	79	66	78	63	18
39	40	62	73	77	54	77	76	86	67	17
40	34	66	72	65	60	65	73	75	63	26
41	46	79	76	73	56	84	74	83	65	24
42	29	60	59	74	61	67	75	80	66	18
43	30	56	49	63	55	56	51	58	52	12
44	30	62	61	74	60	89	75	74	63	18
45	42	66	67	69	54	82	72	92	60	24
46	39	67	72	85	59	66	71	80	70	19
47	33	71	66	79	57	82	73	80	60	17
48	36	53	63	81	54	73	70	76	62	24
49	30	57	68	63	52	58	59	62	63	14
50	34	68	66	72	56	71	76	79	60	21
51	34	65	69	62	53	70	68	79	64	19
52	40	59	58	70	39	82	62	70	62	17
53	45	62	59	71	51	75	70	71	61	22
54	46	64	75	88	46	80	69	81	66	19
55	34	70	71	73	53	74	78	86	64	24
56	41	71	76	81	57	70	77	82	75	25
57	40	70	76	74	47	68	81	84	65	24
58	39	58	66	67	60	79	72	89	71	17
59	47	61	72	83	62	76	79	80	73	21
60	39	64	71	76	55	73	65	74	66	21

The true values of which these are observed values can be deduced theoretically from the structure of the dice throws, each correlation being equal to the number of common dice, divided by the geometrical mean of the two total numbers of dice in the two variates. These true values are as follows* :

TABLE K (formed from Tables F and G).

True Correlations of the Variates.

	x_1	x_2	x_3	x_4	x_5	x_6	x_7	x_8	x_9	x_{10}
x_1	•	·21	·41	·26	·08	·39	·47	·45	·28	·37
x_2	·21	•	·27	·15	·18	·21	·26	·25	·11	·38
x_3	·41	·27	•	·30	·30	·40	·51	·49	·27	·47
x_4	·26	·15	·30	•	·23	·43	·39	·42	·21	·18
x_5	·08	·18	·30	·23	•	·23	·35	·33	·18	·32
x_6	·39	·21	·40	·43	·23	•	·49	·51	·21	·27
x_7	·47	·26	·51	·39	·35	·49	•	·57	·32	·46
x_8	·45	·25	·49	·42	·33	·51	·57	•	·30	·43
x_9	·28	·11	·27	·21	·18	·21	·32	·30	•	·29
x_{10}	·37	·38	·47	·18	·32	·27	·46	·43	·29	•

According to Professor Spearman, if the columns of such a square table of correlations show a correlation with one another, when taken in pairs, which closely approaches perfection, then the variates must contain a factor common to them all, and cannot contain any factors which are common only to a group of them without being common to all.

In the present case, the correlations, by the product-moment formula, of the pairs of columns of true values are as follows :

TABLE L (formed from Table K).

True Intercolumnar Correlations.

Average value = ·64.

	x_1	x_2	x_3	x_4	x_5	x_6	x_7	x_8	x_9	x_{10}
x_1	•	·52	·84	·67	·88	·73	·82	·81	·80	·50
x_2	·52	•	·81	−·01	·59	·20	·61	·51	·66	·82
x_3	·84	·81	•	·63	·64	·69	·92	·86	·93	·65
x_4	·67	−·01	·63	•	·42	·93	·77	·83	·49	·21
x_5	·88	·59	·64	·42	•	·45	·49	·51	·48	·43
x_6	·73	·20	·69	·93	·45	•	·82	·90	·72	·33
x_7	·82	·61	·92	·77	·49	·82	•	·97	·89	·38
x_8	·81	·51	·86	·83	·51	·90	·97	•	·84	·30
x_9	·80	·66	·93	·49	·48	·72	·89	·84	•	·48
x_{10}	·50	·82	·65	·21	·43	·33	·38	·30	·48	•

Although there are group factors, and no general factor, in these variates, and moreover group factors whose influence ramifies in a random fashion determined by drawing cards, yet there is a considerable amount of columnar correlation between the correlations, that is a considerable amount of what has been called "hierarchical order": but it is not perfect; the average of the last table is ·64.

* These have already been published, without the details of their formation, in *Proc. Roy. Soc.* A, xcv. 1919, 404, where they are arranged in another sequence.

Professor Spearman, however, in his psychological experiments has not the true but only the observed correlations. These will differ from the true because of sampling errors, and to correct for this Professor Spearman has proposed, and has used, the formula here called in question, viz.:

$$R'_{ab} = \frac{S(\rho_{xa}\rho_{xb}) - (n-1)\, r_{ab}\overline{\sigma_{xa}\sigma_{xb}}}{\sqrt{\{S(\rho^2_{xa}) - (n-1)\,\overline{\sigma^2_{xa}}\}}\sqrt{\{S(\rho^2_{xb}) - (n-1)\,\overline{\sigma^2_{xb}}\}}},$$

in which from each of the elements of the ordinary product-moment formula there is subtracted a correction. This formula spreads out the observed columnar correlations from zero to infinity, and Professor Spearman then rejects arbitrarily those which are too large, by his requirement that the corrections must not be greater than one-half of the quantities corrected, e.g. $(n-1)\,\overline{\sigma^2_{xa}}$ must not exceed one-half of $S(\rho^2_{xa})$.

In the present instance only three pairs* of columns pass this standard and we get

TABLE M.

Pairs of columns selected by Professor Spearman	Raw observed columnar correlation of these columns	True columnar correlation of these selected columns	As corrected by the Spearman formula
2 and 3	·91	·81	1·15
2 „ 10	·89	·82	1·11
3 „ 10	·96	·65	1·22
Average	·92	·76	1·16

The situation is therefore as follows. These variates really contain no general factor, and do contain random group factors. The true columnar correlations of their correlations average ·64. Professor Spearman's measure of the columnar correlation is 1·16, whence he would deduce that the "hierarchical order" is perfect (he retains correlations which exceed unity without any qualms) and that there are no group factors present, but only a general factor.

I have now completed three examples. One is given on page 360 in Vol. XII of this Journal and is less a random matter than the others. The second is begun there and corrected and completed here with 30 subjects. The third is given here with 60 subjects. All are against Professor Spearman. None has given a result favourable to him, or I would have published it†. I have a second trial of the first example, also against him, and some years ago I made a trial with an example not here mentioned, but with 50 subjects no pairs of columns had passed his correctional standard, and I gave it up.

* In their table, *Brit. Journ. Psychol.* 1912, v, 61, Spearman and Hart include cases where only one or two pairs of columns pass.

† Since the Spearman formula always, or practically always, makes the columnar correlations larger, it must occasionally be an improvement, in those instances where the observed is less than the true. Such instances occur in Table E. But it equally increases those which are too large already.

II. Critical Notice: On the Relation between Home Conditions and the Intelligence of School Children.

By L. Isserlis, M.A., D.Sc. *From data collected by the late* Mrs Frances Wood, B.Sc., *H.M. Stationery Office*, 1923.

(1) This paper is heralded in a preface by Mr Cyril Burt as "an admirable model for the statistical study of such questions" [i.e. the influence of environment and of heredity on the intelligence of school children], and he tells us that the authors "have shown, with unquestionable cogency and clearness, the feasibility and the value of applying such a technique" [i.e. the modern theory of statistics, especially "the statistical device known as 'partial correlation'"]. Mr Burt further informs us that "when all irrelevant factors have been eliminated," Dr Isserlis shows that the correlation between the two items [influence of home conditions and intelligence] lies between ·3 and ·4*. As Mr Burt is careful to tell us that so far there has been practically no scientific basis for a trustworthy opinion on the relation of the intelligence of children to their home environment, we turned with great anticipation to the body of the paper. We shall throughout this notice term it Dr Isserlis' memoir; for notwithstanding that Mr Burt says the impressions formed by the late Mrs Frances Wood were in their general character the same as those extracted by Dr Isserlis from her data, we venture to think that these data only formed a small part of those she *intended* to collect and would have been *forced* to collect when she came to analyse her material and attempted the deduction of conclusions from it. As a matter of fact: *No conclusions whatever can be drawn from the present data as to the relative influence of environment and heredity.*

It is perfectly easy to demonstrate this, for the correlation between the intelligence of *both* parents combined and the intelligence of their offspring is about 0·7; the correlation of parents' intelligence with orderliness of home is certainly above 0·5, and the correlation of home conditions and intelligence of children (according to Mr Burt) is about 0·35. Hence the 'partial correlation' of the child's intelligence with its home conditions for constant intelligence of the parents is of the order zero. It would need a far higher crude correlation between home and child intelligence, as well as a knowledge of two other correlations—nowhere exhibited in Dr Isserlis' memoir, although Mrs Wood clearly saw their fundamental importance—to determine any measure of "the influence" (i.e. as causative factor) of home conditions upon intelligence.

Yet Dr Isserlis does not hesitate to state that:

"On the present data we are entitled to conclude that progressive improvement in home conditions may be expected to react favourably not only on the health but also on the intelligence of school children" (p. 18).

We venture to think that very few if any conclusions at all can be safely drawn from these data; they are wholly inadequate for the end in view. When we read Mr Burt's words about the use of partial correlation and the removal of "all irrelevant factors" we vainly imagined that we should find the higher partial coefficients dealt with†; that we should see the true relation between the child's intelligence and its home conditions corrected for child's age and child's health, for father's habits and his intelligence, and also for mother's health and her intelligence‡.

* He adds: "May we put it, in loose non-technical language, that the importance to the child of social circumstance is as 1 in 3?" We suppose therefore that if the correlation had been ·6 the importance to the child of social circumstance would in non-technical language be one in six, and if the correlation had been 1·0 one in ten! It is somewhat strange that one whose notion of correlation is so vague should have ventured to express a judgment on what is or is not a model of sound statistical treatment!

† Mr Burt seems quite ignorant that such partial coefficients have been used for years in statistico-sociological investigations and apparently supposes the statistical device known as 'partial correlation' "had not previously been used on any scale for problems of this kind" (p. 6).

‡ Of course several of these relations have been long ago discussed and allowed for.

To our great astonishment and real grief, we discovered that Mr Burt's "all irrelevant factors" consist in child's age only, and no partial coefficient beyond that correcting for age is throughout provided. The authors of preface and memoir seem to have therefore a very strange notion of what are or are not "irrelevant factors."

The writer of the preface having cast doubt on all previous investigations in this field, and being himself a psychologist, we turned with interest to the tests for intelligence given in this paper. We were startled, indeed, to find correlations ranging from ·3 to ·6 between the marks obtained in the tests and the age of the child. The main desideratum of a good series of intelligence tests is that it should be free from the age factor*. A series which is correlated with age is really testing knowledge and experience, not intelligence. We know the difficulty of reaching such tests, but we also know that the Stanford Revision in the hands of an experienced and sympathetic examiner does give results singularly free from age correlation. No such care appears to have been applied in choosing or using the tests here adopted, for *the correlation of age with intelligence test marks varies from school class to school class; not even being in every case of the same sign.* Hence it surely follows that these results are either inadequate in number or faulty in method.

(2) Many years ago the present writer drew up a set of categories for the classification of intelligence, and submitted them to teachers for suggestion and emendation. After revision they were applied to over 2000 boys and over 2000 girls by *picked* teachers, the children ranging from 5 to 18. To his great delight the correlation between age and intelligence was found in both cases to be less than 0·1†. In other words the categories represented a scale of intelligence in which knowledge as indicated by age played little part, if the teachers were adequately selected and instructed. In that case, they were practically all university trained men and women.

This scale is known as that of the Biometric Laboratory, and has been largely used to test the relation of home conditions to intelligence on many thousands of children. Accordingly I turned with interest to Dr Isserlis' memoir as our scheme clearly fell under those described in Mr Burt's preface as providing no scientific basis for a trustworthy opinion. Judge of my surprise, when I found my seven categories: Specially Able, Capable, Intelligent, Slow Intelligent, Slow, Slow Dull and Very Dull, *reproduced in each case with only the slightest verbal changes of definition!* It is flattering to know that a scale invented and used twenty years ago can still be looked upon as the best available. It is somewhat quaint, however, to find it used and published without the slightest acknowledgment, and followed by a scale of conditions of clothing, which was used also twelve or fourteen years ago by workers in the Biometric Laboratory to test these very questions of home conditions and intelligence of children, and this on numbers vastly greater than appear in the present paper. One can only suppose that Mr Burt and Dr Isserlis were unaware that Mrs Wood had adopted our scales, and with them "the defects or at least the possibility of defects" which some of our critics think essential to our methods! It is, however, rather a humorous commentary on this "admirable model for the statistical study of such questions" to find it largely based on the categories defined more than twenty years ago by the Biometric Laboratory.

We must sadly confess, however, that, if Dr Isserlis' correlations were valid, the teachers of the present inquiry are not the equals of the teachers of those past times, for our intelligence scale as applied by them instead of giving 0·05 as reached by the teachers of old, gives age correlations varying from −·46 to +·21! In other words, either the correlations are not really significant, or the teachers have not been so chosen and instructed as to be able to distinguish between intelligence and mere knowledge or experience; in particular the results, if accepted,

* This is the essential aim of the Binet-Simon tests and their numerous modifications; yet we read here "the same mental tests were performed by children of all ages" (p. 12), and in this "admirable model," there is no comment on an investigation which sets the same intelligence tests to children of 7—8 and 13—14!

† Namely −0·05 for Boys and −0·08 for Girls, using 2387 Boys and 2241 Girls.

would show that the women teachers of the primary schools were absurdly incapable of using the scale (p. 13). Under the circumstances it is not to be wondered at that relatively low (i.e. as compared with results from other and ampler data) correlations were found between the various methods of estimating intelligence even when corrected for age.

To sum up the section of this memoir dealing with intelligence; the psychological tests certainly did not measure intelligence pure and simple—they were not well chosen—and the teachers' estimate scale—my personal friend of twenty years ago—was possibly not very carefully applied.

(3) Before we pass to the question of environment and intelligence, it seems desirable to test the methods employed by Dr Isserlis and the accuracy with which he has applied them. For upon these conditions must depend the stress we put upon his results. Now a statistician is free to choose his methods, but he should at least give us adequate means of testing what he has done. Unfortunately Dr Isserlis' indications are of the slenderest. He merely tells us "that the 'correlations' employed in the text are product-moment coefficients in the Tables when both categories are measured numerically, except in certain Tables where the regression was obviously not linear; in these the geometrical mean of the correlation ratios has been used. In the Tables where one category only is qualitative the 'correlation' is the correlation ratio (η), while in the remaining Tables where both categories are qualitative it is the contingency coefficient" (p. 28).

Now the correlation ratio requires very special handling, and Dr Isserlis gives us no information in any case as to what has been the exact process which he used. He does not say what test he has adopted for non-linearity, and in most cases the extreme paucity of his data renders any test nugatory. He states nowhere what corrections he has or has not applied to the correlation ratio, or indeed to the contingency coefficient. He does not state how he has given a sign to his correlation working with η, and in certain cases a sign may be justified which is exactly opposite to that given by Dr Isserlis. In no case does he table both η's; and that the geometric mean of the two η's should represent 'correlation' is, I hold, misleading. At any rate such a mean η cannot possibly be used in a linear partial correlation coefficient.

Consider a homoscedastic system*, the regression line of y on x being a parabola of the second order with its axis parallel to y and the y-arrays placed symmetrically about the axis. The regression of x on y will be given by a vertical straight line, or the correlation coefficient is zero. The correlation ratio, $\eta_{x.y}$, is zero also, but $\eta_{y.x}$ can be made as high as we please by reducing the variability of the homoscedastic y-arrays. It is clear that the geometric mean of the η's would give a quite meaningless value of 'correlation,' which would not be increased however we intensified the dependence of y on x. From such an extreme case of divergence in the two η's as this, we can pass through a whole series of cases in which the η's are closer together, but in which their geometrical mean would have no real correlational significance. If the two correlation ratios differ significantly, it is impossible to represent the regression system—and that is the important matter in nearly all these social investigations—by any single constant termed 'correlation,' still less to use this in linear partial correlation coefficients.

But there is a still more important defect, I fear, in Dr Isserlis' treatment in that he gives η for the 'correlation' and attaches to it a probable error. But there is no evidence that he has first investigated a very important matter. If we sample perfectly uncorrelated material, the square of the correlation ratio will always, being a sum of weighted squares, have a finite value. It is necessary in order that an η^2 for a sample should have any importance, that it should differ significantly from $\bar{\eta}^2$, the mean value of η^2 for samples from uncorrelated material. If η^2 be less than $\bar{\eta}^2$, then no purpose is served by attaching to it a probable error†.

Having some doubt as to the legitimacy of some of Dr Isserlis' results, they were tested *de novo*, and finding non-agreement the whole of his tables were reworked by two independent

* The arrays of y only being similar curves with their means along the parabolic regression line.

† Dr Isserlis does not say how he reaches his probable errors for η.

computers. Absolute agreement is of course impossible, when he has given no adequate indication of the route he has followed. He may have combined smaller arrays with adjacent larger arrays, but as the number of his arrays is very small, usually 5 or 6, at most 7, such a procedure would emphasise the importance of using a class-index correction, of which use, however, there appears no evidence. In the following remarks I shall put down (*a*) Dr Isserlis' value of the 'correlation,' the raw η's, the value of $\bar{\eta}^2$, and our corrected values, and shall then draw such conclusions as appear possible from the very meagre data in the tables.

(4) (*a*) *Correlation between School Examination Marks and Intelligence Test Marks.*

Dr Isserlis only gives this for a population of *fifty* (Table 1). He states the 'correlation' to be $+\cdot2745\pm\cdot0882$. Assuming that he worked it by the product-moment method, we find the answer to be $+\cdot3057\pm\cdot0865$. Assuming that he worked it by the correlation ratio method we find*:

$$\eta^2_{x.y}=\cdot1648, \quad \eta^2_{y.x}=\cdot1922,$$
$$\bar{\eta}^2_{x.y}=\cdot1200, \quad \bar{\eta}^2_{y.x}=\cdot0800.$$

The differences here are not really significant on the basis of a population of 50. If we could treat them as such we should have

$$\eta_{x.y}=\cdot4059, \ \eta_{y.x}=\cdot4384, \text{ and } \sqrt{\eta_{x.y}\eta_{y.x}}=\cdot4218,$$

which again does not agree with Dr Isserlis' result and would differ still more had the η's been corrected for class-indices. I think we can only say of this table that 50 observations are quite inadequate to indicate what the nature of the relationship between class-marks and an intelligence testing (depending largely on age) actually is. No table for examination marks and age being given, one cannot correct for age.

(*b*) *Correlation of Intelligence Test Marks and Teachers' Estimate of Intelligence.*

Dr Isserlis' Tables 2—4 give groups of girls numbering 50, 48 and 63, and his Table 5 pools them and gives 161 individuals. His Table 6 pools straightway 106 boys from standards 4, 5 and 7.

We presume he has worked with $\eta_{y.x}$.

We have

Table	Dr Isserlis	Our crude value $\eta_{y.x}$	$\eta^2_{y.x}$ relative to $\bar{\eta}^2_{y.x}$	Class-Index corrected $\eta_{y.x}$	Fully corrected $\eta_{y.x}$
2	$+\cdot4720\pm\cdot0707$	$(+)\ \cdot4785$	Not significant	·5019	·4654
3	$+\cdot6115\pm\cdot0610$	$(+)\ \cdot4712$	Not significant	.4919	·4530
4	$+\cdot5698\pm\cdot0574$	$(+)\ \cdot5798$	Significant ...	·6366	·6157
5	$+\cdot4407\pm\cdot0427$	$(+)\ \cdot4497$	Significant ...	·4679	·4500
6	$+\cdot4702\pm\cdot0510$	$(+)\ \cdot4750$	Significant ...	·4920	·4578

We are unable to say why neither our crude values nor our corrected values of the 'correlation' have any show of agreement with Dr Isserlis' values. In Tables 2 and 3 the frequency is so small that the correlation ratio is not significant as compared with its random value.

To test the sign of the 'correlation' we used biserial r. Thus for example in Tables 3 and 4 dividing between C and D

$$r=+\cdot2211\pm\cdot1195,$$
$$r=+\cdot2655\pm\cdot1168.$$

These sufficed to show (*a*) that the regression is far from linear, (*b*) that there is some reason to be sceptical even about the result for Table 4 being significant, if we consider it merely from

* $\eta_{x.y}$ is the correlation ratio for horizontal, $\eta_{y.x}$ for vertical arrays for Dr Isserlis' tables.

the standpoint of the correlation coefficient. Clearly except in the case of all the classes clubbed together, i.e. Tables 5 and 6, we have material far too inadequate to permit of our drawing definite conclusions. Tables 5 and 6 were tested by biserial r. The correlation coefficients were respectively

Table 5 : $r = \cdot 4874 \pm \cdot 0552$,

Table 6 : $r = \cdot 5402 \pm \cdot 0633$.

We cannot expect r found in this manner to be always less than η, but taken in conjunction with η the results are sufficient to indicate that there was definite correlation, approximately linear, between the tests and the teachers' estimation of the order ·45 to ·50.

It is desirable to note that correlations have been found and published a number of years ago for children between such characters as the teachers' estimate of intelligence, and place in form, place in examination and marks in examination, the examination not generally being marked or set by the class teacher. When the correlations for these different measures of intelligence were corrected for *both* age and class (or form), and this on considerably larger numbers than those of Dr Isserlis, the values found ranged from ·6 to ·9, showing considerably higher values than those found from the present intelligence tests and the teachers' estimate*.

Recent tests between the Teachers' Estimate of Intelligence (the same scale as used by Mrs Wood) and the Intelligence Quotient (with the Stanford Revision) indicate no correlation of this quotient with age in two boys' schools, although some existed in girls' schools, and very much higher correlations between both measures (·6 to ·8) than flow from Mrs Wood's intelligence tests in which there was no age differentiation of tests. The defect of relationship in the data dealt with by Dr Isserlis is not I think due to the two types of test estimating different characters, but to the psychological error resulting from neglecting the fundamental idea of all Binet-Simon test series and putting the same questions to all children from 7 to 14 years of age. This error seems to me to rule out all possibility of useful conclusions being drawn from the psychological tests applied in this investigation.

(*c*) *Correlation of Teachers' Estimate of Intelligence with Age.*

Tables 7—19 of the memoir deal with the relation of the two intelligence tests to age.

We take first age and the Teachers' Estimate of Intelligence.

Class	Table	Dr Isserlis	Our crude value $\eta_{y.x}$	$\eta^2_{y.x}$ relative to $\bar{\eta}^2_{y.x}$	$\eta_{y.x}$ corrected for Class-Index	$\eta_{y.x}$ fully corrected
Girls : 6	7	$-\cdot 4606 \pm \cdot 0752$	$(-)\cdot 4713$	Not significant... ...	·4944	·4569
4	8	$-\cdot 2382 \pm \cdot 0916$	(?) ·2651	Not significant... ...	$\eta^2_{y.x}$ less	—
1 and 2	9	$+\cdot 2027 \pm \cdot 0815$	$(+)\cdot 1996$	Not significant... ...	than $\bar{\eta}^2_{y.x}$	—
1, 2, 4 and 6	10	$+\cdot 1296 \pm \cdot 0522$	$(+)\cdot 1320$	Not significant... ...	·1373	Imaginary
Girls, Whole School	11	$+\cdot 2152 \pm \cdot 0334$	$(+)\cdot 2215$	Possibly just significant	·2275	·2021
Boys, Whole School	12	$+\cdot 1597 \pm \cdot 0418$	$(-)\cdot 1616$	Not significant... ...	·1669	·1037

The data are so sparse and the 'correlation' so low that it can only be said of *one* of these results that it is possibly just significant. All the others are not significant when we compare $\eta^2_{y.x}$ with its random value, $\bar{\eta}^2_{y.x}$, on the assumption of no correlation. There is thus no ground whatever for asserting definitely that the Teachers' Estimate was influenced by the age of the child. It may be that sparsity of material and not want of judgment in the teachers will account for Dr Isserlis' results.

Tables 13 and 14 deal with separate standards of the whole boys' school. Dr Isserlis gives the values :

$$+\cdot 0334 \pm \cdot 0552 \text{ and } +\cdot 1816 \pm \cdot 0634$$

* See *Biometrika*, Vol. VII. pp. 542—8, and Vol. VIII. pp. 79—93; the latter paper is apparently the source from which Mrs Wood drew her "Intelligence Scale."

against our $\eta_{y.x}$, (−)·2306 and (+)·1846 for crude values, (−)·2373 and (+)·1913 corrected for class index and (−)·1693 and imaginary for full correction.

But $\eta^2_{y.x}$ is not significantly different from $\bar{\eta}^2_{y.x}$ in the first and is less than $\bar{\eta}^2_{y.x}$ in the second. Accordingly we conclude either that there is no correlation, or the data provided are inadequate to determine it. There is no solid foundation whatever for the statement (p. 13) that:

"in the lower classes the younger children and in the upper classes (to a slight extent) the older children are considered by their teachers to be more intelligent,"

and the 'partial explanation' suggested is unnecessary.

We notice again that in no single case does Dr Isserlis' value agree with either our crude value or our corrected values. He appears without doubt to be calculating or correcting the correlation ratio in a manner unfamiliar to us.

(*d*) *Correlation of Intelligence Tests with Age.*

We now turn to the correlation of Intelligence Tests with Age of Child.

Class	Table	Dr Isserlis	Our Product-Moment Value	Geometric Mean of crude η's
Girls: 6	17	+ ·2817 ± ·0878	− ·2203 ± ·0908	·3163
4	16	+ ·3099 ± ·0880	+ ·1485 ± ·0952	·3144
1 and 2	15	+ ·3364 ± ·0754	+ ·2008 ± ·0816	·3482
Girls: 1, 2, 4 and 6 ...	18	+ ·5045 ± ·0395	+ ·4636 ± ·0417	·5039
Boys: Standards 4, 5, 7	19	+ ·6394 ± ·0387	+ ·6072 ± ·0414	·6535

All our values differ from Dr Isserlis', one even in sign. These differences are so great, that as the tables do not appear to give linear regression, we determined the correlation ratios both ways and took the geometrical mean of these results as indicated in the last column. Our results are now nearer to Dr Isserlis', and he may have reached them by this pathway—which is, I think, an erroneous one. But if this method be adopted, both η's in Table 17, both η's in Table 16, and one of the η's in Table 15 must be rejected as insignificant, or we must admit that no argument can be based on the η's derived from tables containing only 50, 48 and 63 observations. This thrusts us back on the product-moment coefficients, and even here if our values and not Dr Isserlis' be correct, one of these correlations is not significant and the other two are only just significant, if that. Thus we are compelled to conclude that the data for the intelligence tests in the individual groups are wholly inadequate to establish any relation between age and intelligence, one result being insignificant and of the two just possibly significant one is positive and the other negative.

Taken for *pooled* groups significance is really obtained in the case of girls (and possibly in that of boys*) by pooling heterogeneous and insignificant material.

(*e*) On the basis of the insignificance shown in our tables in (*b*), (*c*) and (*d*) we do not feel able to provide partial correlations to the extent given by Dr Isserlis on his p. 14. Where a correlation is insignificant we do not see that we can do anything better than put it zero. In the cases of the small groups of girls we decline to hazard any values for the partial correlations of Teachers' Estimate and Intelligence Tests. For the Table 10 and Table 12 material, all we can do is to put the correlation of Teachers' Estimate of Intelligence and age zero, we thus have for comparison:

* There are no group tables for boys.

Correlations of Teachers' Estimate and Psychological Tests of Intelligence.

Class	Dr Isserlis	Our 'best' suggestions
Girls, 1, 2, 4 and 6 ...	$+ \cdot 4450 \pm \cdot 0613$	·5079
Boys, Standards 4, 5, 7	$+ \cdot 4682 \pm \cdot 0512$	·5762

Our values on this data approach nearer, but fall short of, the values recently found (·6 to ·8) between Stanford Revision Tests and Teachers' Estimates. But our 'best' suggestions are only really guess-work, they are based on inadequate material and a compounding of heterogeneous constituents.

(*f*) We now turn to the main results of the paper, those dealing with the correlation of intelligence and environmental conditions. There are three conditions suggested for consideration.

Social Status of Parents.

This is given by a scale of five classes *A*, *B*, *C*, *D*, *E*, which is quite good of its kind. I do not know its origin though it shows traces of both Booth's and Rowntree's scales.

Clothing in the categories already referred to.

Care of Home. This is given in five classes with three intermediate classes *A*, *B*, *C*, *D*, *E* and *A* −, *B* −, *C* −. No categories are stated and the *A* − and *B* − (*minus*) classes were obviously badly determined for statistical purposes. We are told (p. 16) that the grading was done carefully by members of the Care Committees.

In medias res (p. 16) we are suddenly introduced to a new factor, or one with a new name, *Economic Position*. There appears to be no account of how the economic position was determined, and possibly it is only using another phrase for *Social Status* (which also reappears as *Social Position* in the heading of the tables. If so there are only three environmental factors considered of which two, *Clothing* and *Social Status*, were estimated by the teachers themselves; the third *Home Care* was provided by the Care Committees, and the nature of the graduation is not stated.

How far are these characters of the home independent environmental factors? No attempt has been made by Dr Isserlis to answer this point; there are no correlation tables provided between them. Nor again has any attempt been made to correlate the *Cleanliness* of the children with the Care Committees' graduation of *Home Care*. But one result of examining the sources of knowledge as to the children is clear. More than half the information as to Intelligence came from the teachers and two-thirds of the data as to environment. Mr Burt suggests that it is possibly defective to permit the teacher to assess both the intelligence of the child and its general social status. And that it was owing to this doubt that the present inquiry was started in 1913, with an elaborately controlled inquiry into general intelligence and social conditions. Where no correlations are given between the estimates of the teachers as to social status and the opinions of the Care Committees it is very difficult to perceive how the inquiry is 'controlled.' With the control in the matter of estimates of intelligence we have already dealt, and indicated that it is based on far too slender material and too defective psychological tests to reach anything like an adequate standard of correlation between (i) the careful judgment of a highly trained teacher and (ii) a revised form of Binet-Simon individual tests.

(*g*) *Influence of Age on Environmental Factors.*

Only five such tables are given. They do not include any for *Social Status*, or *Home Care* in the case of Girls; there are tables for Boys' *Age* with *Economic Position* and with *Home Care*, and three tables for *Age* with *Clothing*.

Table	Class	Subject	No.	Dr Isserlis	Raw η	Corrected η
25	Girls, Whole School ...	Clothing and Age	372	$+\cdot3271\pm\cdot0312$	·1905	·2185
26	Girls, Classes 1, 2, 4 and 6	" "	162	$+\cdot3047\pm\cdot0481$	·3271*	·3407
36	Boys, Standards 4, 5, 7 ...	" "	104	$+\cdot1462\pm\cdot0647$	·1486†	·0553
29	Boys, School H	Economic Position and Age	247	$-\cdot1381\pm\cdot0421$	$-\cdot1398$‡	Not significant
37	Boys, School H, without Special Class	Home Care and Age	255	$+\cdot0783\pm\cdot0420$	$+\cdot0783$‡	Not significant

Since one variate is quantitative and the other qualitative we might assume Dr Isserlis used the correlation ratio (see his p. 28), at any rate in Tables 29 and 37. In Tables 25 and 26 there are respectively only three and two arrays. The latter would give a very poor value of the 'correlation' using the correlation ratio, and the former would certainly need the class-index correction. For the first time, one of our numbers (that for *Home Care* and *Age*) agrees absolutely with Dr Isserlis' for the raw η. Of the five cases three are not significant, one the Girls of Classes 1, 2, 4 and 6 gives a value of η^2 which is possibly just significant, and only the whole school of girls shows some improvement in clothing with age. This fact had already been noted by Dr Heron, i.e. that Girls' Clothing improved with age while Boys' did not. On the whole we can say that the data are insufficient to show by the processes adopted any improvement in environmental conditions with age, and except possibly for Girls' Clothing no correction can be legitimately made for age in the correlations between intelligence and environmental conditions on the present data.

(*h*) *Influence of Environmental Conditions on Intelligence.*

(i) Intelligence Tests and Clothing.

The tables are 27 for 162 Girls and 28 for 104 Boys. Presumably these were reduced by $\eta_{y.x}$, although one table consists of three columns and the other of four of which one contains only 2·5 and another 10 individuals.

		Girls	*Boys*
Dr Isserlis gives :		$+\cdot4437\pm\cdot0426$	$+\cdot2958\pm\cdot0604$
We find :	Raw $\eta_{y.x}$	(+)·4412	(+)·2727 (Not significant)
	Corrected $\eta_{y.x}$	(+)·5281	(+)·2774
	Biserial r	$+\cdot5106\pm\cdot0536$	$+\cdot2102\pm\cdot0815$

Again our values differ from Dr Isserlis'. We cannot definitely assert the existence of a relationship between intelligence and clothing in boys using the η-method, but it exists in girls. A safer method of approach seems 'biserial r.' This shows quite a definite correlation in the case of girls, and a possible but not certain correlation in boys. The great difference between the two sexes should make one pause before asserting that clothing is evidence of home care; there is clearly a very large factor of another kind. The present data are not adequate to clear this up.

(ii) Intelligence Tests and Economic Position.

One table only, Table 32, is provided with the wholly inadequate number, 98, of boys. I have said that I do not know whether Economic Position is the same character as *Social*

* If we took Dr Isserlis literally the correlation ratio $\eta_{y.x}$ would be ·2439; we have given $\eta_{x.y}$ from 'the biserial η' method, 'biserial r' is here probably the best method and gives $\cdot3102\pm\cdot0623$, better clothing with greater age.

† $\eta^2_{y.x}$ is *not* significant compared with $\overline{\eta}^2_{y.x}$ and accordingly we cannot assert any relation between boys' age and their clothing.

‡ $\eta^2_{y.x}$ is not significant when compared with $\overline{\eta}^2_{y.x}$ and negative in the latter if fully corrected.

Status. There are five categories *A*, *B*, *C*, *D*, *E* and the inadequacy of the material may be illustrated by the fact that there are only three boys in *A* and eight in *E*, nearly 50 °/₀ being in *C*. Dr Isserlis gives the correlation $+\cdot1941 \pm \cdot0656$, presumably working with $\eta_{y.x}$, we find:

Raw $\eta_{y.x}$	Corrected $\eta_{y.x}$
$(+)\cdot1941$*	$(+)\cdot1408$.

But $\eta^2_{y.x} = \cdot037{,}679$,

while $\bar{\eta}^2_{y.x} = \cdot040{,}816$,

i.e. the mean value of $\eta^2_{y.x}$ for random samples of uncorrelated material is greater than the observed value of $\eta^2_{y.x}$, or there is no significance whatever in the observed result. Thus the Intelligence Tests again fail to give anything but a negative result owing to inadequacy of material.

Dr Isserlis now gives an illustration of how great a caution ought to be used even by a highly trained and distinguished statistician.

Having found for School H the following correlations:

Table 32. Intelligence Tests and Economic Position: $+\cdot1941 \pm \cdot0656$,
Table 19. Intelligence Tests and Age: $+\cdot6394 \pm \cdot0387$,
Table 29. Age and Economic Position: $-\cdot1381 \pm \cdot0421$,

he proceeds to calculate a partial correlation coefficient and finds (p. 17):

Intelligence Tests and Economic Position: $+\cdot3708 \pm \cdot0588$,

a result nearly double the total correlation and with a less probable error.

Now Table 32 is based on 98 boys, Table 19 on 106 boys and Table 29 on 247 boys. In other words 140 new boys are introduced into Table 29 who are not included in the other tables! What new elements this addition of 140 boys not tested for intelligence may have introduced cannot be determined; these boys may not have been the same age nor have had the same distribution of economic position. But it is not of much importance, for we have seen that the results $+\cdot1941$ and $-\cdot1381$ are both of no significance, and accordingly all we can say of them is that we have no evidence on the data that they differ from zero†. It is impossible from components which are of no significance to reach a partial coefficient of ·3708 and fortify it by a probable error between only a fifth and a sixth of its total value.

(*i*) *Teachers' Estimate of Intelligence and Home Conditions.*

We can cite here results for Teachers' Estimate of Intelligence and *Social Position*, *Clothing*, *Home Care* and *Economic Position*. All these cases fall into the contingency method according to Dr Isserlis' statement, and therefore should prove easy of testing. We have the results given in the table on the following page.

It will be noticed that the psychological tests have only been used in three cases to discover the relations of intelligence to home environment. In two of these, Boys' *Clothing* and *Economic Position*, they give no significant results; in the third instance, Girls' Clothing, they give a significant result. But the correlation between girls' intelligence and clothing is not a novel result—it had already been reached on wider material by Dr Heron in 1910‡, and what is more he commented very wisely on it in the following manner:

* This is only the second occasion on which we are able to agree with a correlation value given by Dr Isserlis. It leads us to believe that he was working with 'raw' η values.

† There is more than a suspicion that the high value for Age and Intelligence Tests of Table 19 has arisen from heterogeneity which, as in the case of girls, is likely to occur when the same tests are used at all ages.

‡ *The Influence of Defective Physique and Unfavourable Home Environment on the Intelligence of School Children*, Cambridge University Press, 1910.

After noticing the differences between the two sexes, the Girls having higher correlation than the Boys, and after commenting on the variation of the correlation with the schools, he writes: "It seems impossible therefore to use the condition of the clothing as an effective test of home environment so long as the source of these inconsistencies is undetermined. It seems more reasonable to suppose that mental capacity (either directly or because the abler child is the child of abler parents) produces neater clothes and that this is far more the rule with girls than with boys. It would be difficult even with the steady values obtained for girls to assert that good clothing was a sign of good home environment and that that was associated with intelligence. The orderly-minded parents may produce an orderly-minded child as well as provide orderly attire for their children, and the fact that even intelligent and mentally active boys will 'tear their clothes to pieces' may explain this sex-difference and show how little service a school appreciation of clothing may sometimes be as a measure of home environment. The actual survey of the homes is the only way to reach the truth on this question of the condition of the clothing as a measure of home environment, and on the influence of the latter, apart from heredity, on mental capacity." Pp. 47—48.

Table	Class	Teachers' Estimate of Intelligence with	No.	Dr Isserlis	Our Conclusions
20	Boys, Schools C and B	Social Position	309	$+\cdot3671\pm\cdot0430$	$C_2=\cdot3675$ crude; $=\cdot4007$ corrected, but scarcely significant*
21	Girls, Class 9	„ „	39	$+\cdot2583\pm\cdot1341$	Insignificant and really unworkable†
22	Girls, Classes 3, 5, 9	„ „	155	$+\cdot3047\pm\cdot0655$	$C_2=\cdot5507$, $\eta_{x.y}=\cdot5287$ Both significant
24	Girls, Classes 1, 2, 4, 6	Clothing	161	$+\cdot3505\pm\cdot0466$	$C_2=\cdot3628$ crude, but not significant ‡. Biserial $\eta_{y.x}=\cdot3391$
23	Girls, Whole School	„ „	372	$+\cdot3758\pm\cdot0300$	$C_2=\cdot3270$ crude, but not significant §
30	Boys, School H	Economic Position	247	$+\cdot2646\pm\cdot0399$	·3401, but not significant‖
31	„ „	Care of Home	247	$+\cdot3841\pm\cdot0366$	·4518, but not significant¶
33	„ „	Care of Home and Standard	—	$\cdot1602\pm\cdot0412$	$C_2=\cdot2818$ crude, but not significant**

I have added the last case because Dr Isserlis appears to lay some weight on it.

The present memoir adds nothing in this respect to the solution of the problem as stated by Dr Heron.

In the preface to the present memoir we have been told that the psychological tests were to be used as a fundamental control of the whole series of observations, because doubts have been cast on the Teachers' Estimates. We have seen that they, the Intelligence Tests, have not,

* $\phi^2=\cdot156072$, $\bar\phi^2=\cdot110{,}032$ and $\sigma_{\bar\phi^2}=\cdot0271$.

† 30 out of 39 girls are in *one* category of Social Position; and 9 out of 12 occupied cells contain one unit or two units!

‡ $\phi^2=\cdot151{,}538$, $\bar\phi^2=\cdot105{,}590$ and $\sigma_{\bar\phi^2}=\cdot0362$, which is not definitely significant.

§ $\phi^2=\cdot119{,}760$, but $\bar\phi^2=\cdot091{,}398$ and $\sigma_{\bar\phi^2}=\cdot0271$.

‖ $\phi^2=\cdot130{,}796$, but $\bar\phi^2$ or mean value for zero contingency is $\cdot097{,}166\pm\cdot0573$. Classes *D* and *E* and *F* and *G* pooled to avoid units.

¶ $\phi^2=\cdot256{,}526$, but $\bar\phi^2$ or mean value for zero contingency is $\cdot222{,}672\pm\cdot0857$. On the other hand a just significant result, $\cdot3076\pm\cdot1139$, can be found by a fourfold table, division between *B* and *C* for Home Care and *C* and *D* for Intelligence.

** $\phi^2=\cdot086{,}263$, but $\bar\phi^2=\cdot0902$, and accordingly ϕ^2 is less than $\bar\phi^2$. Dr Isserlis states that the result is significant.

owing to their slender character, thrown any significant light on the relation of intelligence to home environment. We are in fact thrown back on precisely the same manner of treating environment and intelligence that had previously been used in a considerable number of memoirs.

Dr Isserlis appears in each case to have attached to his Contingency Coefficient the probable error of the product-moment value of the corresponding coefficient of correlation. This gives an air of significance to these contingency coefficients, which they do not really possess. In order that a contingency coefficient may be significant it is essential that ϕ^2 should differ significantly from $\overline{\phi}^2$, the mean square contingency for zero association in the same table. We cannot by this test assert definite significance for six of the seven tables, which Dr Isserlis has discussed by this method. Our contingency values differ in practically every case from his, and in some cases widely. We cannot, however, say what cells he may have grouped together to obtain his results. Nevertheless we are practically certain—as our values for the tables as they stand are usually higher than his, or at least of the same order—that no reliably significant results can be obtained from his data, except in the third of his Tables (22) correlating *Social Position* with Teachers' Estimate of Intelligence in three combined classes of school girls. It may well be that another treatment of the tables, say as fourfold tables or two-row (or three-row) η tables, would have led to significant results, but the statistical treatment in this paper by no means justifies us in asserting that the order of the correlation between intelligence of the child and the home conditions is definitely of the order ·35. It possibly is of such an order, judged by the fact that the correlation of the Teachers' Estimate of Intelligence and the Teachers' Estimate of Clothing, worked out with exactly the same categories and on adequate material—1725 Boys—in 1911 by Mr W. H. Gilby, shows correlations of this order*, and therefore probably of a still higher order in Girls. Well, let us admit that the correlation of home conditions and child's intelligence is significant and of the moderate order ·35—although it is not demonstrated in this memoir—and then what follows from it? Why, that unless Dr Isserlis can prove that social status and home care are correlated with a lower intensity than 0·5 with the intelligence of the parents, and this he will find hard to do†, then the intelligence of the child will have a zero association with its home environment for constant intelligence of the parents.

It appears to me impossible to admit Dr Isserlis' statement that

"on the present data we are entitled to conclude that progressive improvement in home conditions may be expected to react favourably.....on the intelligence of school children" (p. 18).

Such a conclusion would only be possible if he had removed "all irrelevant factors"; he has only removed age which had been injudiciously introduced by setting the same psychological tests to children of all ages. Such an "irrelevant factor" as intelligence of the parents he has not considered at all, and until he has done so, no such conclusion as the above can possibly be drawn from his categories, even if they contained adequate frequencies to admit of definitely significant correlations being found.

We do not think full justice has been done to Mrs Wood in the matter. I have not the slightest doubt that had she lived she would have continued to collect material, or if she had attempted to reduce it in its present state, she would at once have been convinced of its inadequacy. She realised the importance of the heredity factor; even Mr Burt seems to recognise it also, although he proceeds to speak of Dr Isserlis' work as "an admirable model for the statistical study of such questions." I venture to think it is not, and that it is lacking in the "unquestionable cogency and clearness," which Mr Burt attributes to it.

The main thesis that the correlation of the home environment and the intelligence of the

* *Biometrika*, Vol. VIII. pp. 94—108. It is a pity Dr Isserlis did not examine the literature of the subject, or I venture to think he would hardly have sanctioned Mr Burt's preface.

† The high correlations (·6 to ·8) of parental habits with home conditions, and of wages with skill in occupation (another measure of paternal intelligence) justify me in this remark. I should not, indeed, be surprised to find the partial correlation negative.

child lies between ·3 and ·4 is undemonstrated, and, if it had been, it would not have justified Dr Isserlis' conclusion.

Dr Isserlis is a very expert mathematical statistician, but it is difficult to approve his numerical handling of even the meagre data of this paper.

A Government Department has always a difficult task, when it comes to the state publication of a scientific paper, and the Medical Research Council in publishing this under the title of *Privy Council* may not be supposed to give their approval or that of the more august body to the conclusions professed to be reached. Nevertheless there is grave danger in a work thus given an *imprimatur* and thus prefatorily heralded. Belief will be given by many to statements which are not demonstrated on the ground alone that they are published officially. That is the danger which always attends scientific research under the aegis of a government department. I feel this so strongly that I have written at greater length than the mere fact of inadequate handling of meagre data would at first sight seem to justify.

I can only trust that my friends Dr Greenwood and Dr Isserlis will accept my criticisms recognising the standpoint from which I have written them—namely that of nearly twenty years' experience of work at these very topics, which has led the Galton Laboratory to be very conscious of the statistical pitfalls which beset the paths traversed by Dr Isserlis, of the urgent need for large numbers, and of the great weight of material which leads to conclusions wholly contrary to those propounded by the writer of the present paper.

K. P.